The Political Economy of Social Identity in 19TH Century Germany

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Abstract English

This dissertation is composed of four chapters which deal with various aspects of social identity in 19th century Germany. The first chapter analyzes the effect of nation-building in cities that became part of Prussia in 1815. Data on first name choices by parents in eight German cities allow to elicit changes in national identity. Using within-family variation and comparing cities that become part of Prussia with other cities that stayed outside Prussia identifies the overall positive treatment effect. The second chapter investigates the effect of Bismarck's carrot and stick policies on the electoral success of the socialist party. For identification, I exploit spatial and industry specific variation in treatment intensity due to ex-ante existing local health insurance and use a difference-in-differences approach as well as shift-share instrument. The results show that Bismarck failed in reducing the support for the socialist party; to the contrary, the socialist party gained in constituencies more affected by his policies. The third chapter studies the "China shock" of the first globalization (1880-1913) – the "grain invasion" – coming from the Americas into Europe. The empirical results show that trade shocks in agriculture depress the economy of rural counties in Imperial Germany. Crucially, there is no indication of a corresponding decline in income per capita or a rise in political polarization which is attributed to high levels of labor migration. The fourth chapter revisits Max Weber's hypothesis on the role of Protestantism for economic development in its contemporary context. The empirical analysis provides new and direct evidence that Protestantism neither mattered for income levels, nor savings, nor literacy rates across Prussian counties after 1870. Instead, the chapter argues that nationalism is crucial for both the interpretation of Weber's Protestant Ethic and empirical tests thereof. While covering quite different contexts in 19th century Germany, these chapters are united in dealing with various aspects of social identity – either exploring potential political and economic causes of changes in social identities (chapter 1, 2, and 3) or possible consequences of social identity (chapter 4).

Abstract German

Diese Dissertation besteht aus vier Kapiteln, die sich mit verschiedenen Aspekten sozialer Identität im Deutschland des 19. Jahrhunderts befassen. Das erste Kapitel analysiert die Auswirkungen der Nationsbildung in Städten, die 1815 Teil Preußens wurden. Daten über die Vornamenswahl der Eltern in acht deutschen Städten und die Klassifizierung der Vornamen erlauben es, Veränderungen der nationalen Identität zu fassen. Anhand der Variation in Familien über die Zeit und des Vergleichs von Städten, die Teil Preußens werden, mit anderen Städten, die außerhalb Preußens blieben, wird der insgesamt positive Effekt ermittelt. Das zweite Kapitel untersucht die Wirkung von Bismarcks Zuckerbrot-und-Peitsche-Politik auf den Wahlerfolg der Sozialdemokratie. Zur Identifizierung der kausalen Effekte nutze ich die räumlichen und branchenspezifischen Unterschiede in den vorher bereits bestehenden lokalen Krankenversicherungen sowie detaillierte Informationen zu verbotenen Vereinen in einem Differenz-in-Differenzen Ansatz. Die Ergebnisse zeigen, dass es Bismarck nicht gelungen ist, die Unterstützung für die Sozialdemokratie zu verringern; vielmehr gewann die Sozialdemokratie in den von seiner Politik stärker betroffenen Wahlkreisen. Das dritte Kapitel untersucht den „China-Schock“ der ersten Globalisierung, die „Getreideinvasion“. Die empirischen Ergebnisse zeigen, dass Handelsschocks in der Landwirtschaft die Wirtschaft der ländlichen Kreise in Preußen belasten. Entscheidend ist indes, dass dieser Handelsschock nicht zu einem entsprechenden Rückgang des Pro-Kopf-Einkommens oder einer Zunahme der politischen Polarisierung führte. Grund hierfür ist das hohe Level an Arbeitsmigration. Im vierten Kapitel wird Max Webers Hypothese einer protestantischen Ethik überprüft. Die empirische Analyse liefert Evidenz dafür, dass Protestantismus nach 1870 weder für das Einkommensniveau noch für die Ersparnisse oder die Alphabetisierungsraten in den preußischen Landkreisen eine Rolle spielte. Stattdessen ist, so das Argument, Nationalismus sowohl für die Interpretation von Webers protestantischer Ethik als auch für deren empirische Überprüfung von entscheidender Bedeutung. Obwohl diese Kapitel ganz unterschiedliche Kontexte im Deutschland des 19. Jahrhunderts abdecken, eint die Kapitel, dass sie sich mit verschiedenen Aspekten der sozialen Identität befassen - entweder mit den möglichen politischen und wirtschaftlichen Ursachen des Wandels sozialer Identitäten (Kapitel 1, 2 und 3) oder mit den möglichen Folgen sozialer Identitäten (Kapitel 4).

Overview

This dissertation consists of four papers.

Chapter 1 "**On the origins of national identity. German nation-building after Napoleon**" is jointly written with Nikolaus Wolf. An older version is published as *CRC TRR 190 Discussion Paper No. 217*.

Chapter 2 "**When autocrats fail: Bismarck's struggle against the socialists**" is single-authored. There is no published version of this paper yet.

Chapter 3 "**Migrating from the grain invasion: Trade shocks, labor markets and political polarization in Imperial Germany, 1880-1913**" is jointly written with Richard Bräuer and Wolf-Fabian Hungerland. There is no published version of this paper yet.

Chapter 4 "**Weber revisited: The Protestant Ethic and the Spirit of Nationalism**" is jointly written with Iris Wohnsiedler and Nikolaus Wolf. This paper appeared in The Journal of Economic History (Vol. 80 No. 3, 710–745) published by Cambridge University Press. The Copyright holders are Cambridge University Press and the Economic History Association. The article is reproduced with permission, not for further re-distribution or re-use. Previous versions were published as *CEPR Discussion Paper No. 14963*, *CESifo Working Paper No. 8421*, and *CRC TRR 190 Discussion Paper No. 199*.

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Felix Kersting
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Contents

List of Figures

List of Tables

Introduction

On the eve of World War I, the two largest mass organizations of Imperial Germany, the trade unions and the nationalist *Kriegervereine*, had a combined membership of about six million persons (relative to a population of 64 million). Both organizations had basically no members when the Empire was founded in 1871. More than 400.000 workers – often organized in the trade unions – went on strike in 1905 for higher wages. Meanwhile, an unknown number of members of the *Kriegervereine* practiced shooting and war every Sunday. In 1907, almost 50.000 pupils of the Polish-speaking minority in the province of Poznan went on strike in protest against the fact that an ever-increasing number had to take religious instruction in German. Until the 1870s, no such conflicts occurred. Even on an personal level – namely the choice of first names – changes become apparent: while on average only 13% of all children had national first names (excluding family traditions in first names) in 1810, this share increased to 23% 60 years later.[1] These are first signs that new social identities and related conflicts gain importance during this century. These social identities, their economic and political causes as well as economic consequences are the subject of this dissertation.

Social identity is defined here as the idea that people value their membership in social groups, and that such valuations are interdependent and changing over time (Tajfel and Turner, 1986).[2] These changes in social identities also show economic significance: From drastic examples such as the (partial) enthusiasm for World War I over the demand for more protectionism to the willingness to strike for higher wages. There are numerous important questions related to this topic: Was the nation-state able to shape its citizens by using different tools like propaganda or repression or using new policies like redistribution? Which tools were most effective? Which role did economic shocks play in changing social identities? Did social identities (and conflict between them) matter for regional development? While historians have been eager to address these questions, not least in the *Sonderweg* debate, empirical evidence on these questions remains scant and unsatisfactory. An empirical angle, combined with knowledge about the historical

[1] For membership in the nationalist *Kriegervereine* see Rohkrämer (1990, p.272). For membership in trade unions and strike activity see Hohorst *et al.* (1975, p.136ff.). For details on the school strike in Poznan see Lamberti (1989, p.143ff.). For the development of first names see chapter 1.

[2] Identity has become a major field of research in economics, starting with the work by Akerlof and Kranton (2000). For a recent overview, see Shayo (2020) and Kranton (2016), for a critical discussion see Davis (2011). Overall, this richer notion of the individual (in comparison to the standard atomistic individual) allows incorporating a social dimension in economic analysis.

context, rather than a purely historical one, helps to understand the relative significance of events, evolutions, and causes. In this sense, the four chapters of this dissertation aim to illustrate the manifold contributions that economic history research can make to a better understanding of social identities and changes thereof.

In the first chapter, "**On the origins of national identity. German nation-building after Napoleon**", Nikolaus Wolf, and I investigate the effect of shaping social identities by national identity policies at the end of Napoleon's era when the Prussian king first propagated the idea of a "German nation". While historians agree that 'Germany' did not exist in early 19$^{\text{th}}$ century, there are numerous debates on the timing and reasons for the spread of nationalism in this period.[3] So, without any doubt, national identities were formed by many factors and over long periods of time. Here, we focus on one particular period, and one particular mechanism, for which we provide causal evidence at the level of individuals. The period is the Napoleonic wars and their aftermath, and the mechanism refers to manipulative elites. Specifically, we compare changes in social identities in cities that became Prussian in 1815 after the Peace of Vienna to changes in cities that stayed outside Prussia. The empirical challenge we face in this analysis is twofold: how to capture (national) identity in early 19$^{\text{th}}$ century Germany in a quantitative analysis? How to deal with the lack of control variables? We address the first challenge by using the parents' decision on first names for their children as an indicator of their social identity. To differentiate between different social identities, we classify first names based on their origin in national, religious, European, and ancient first names. Thus, our indicator for the national identity of parents is a nationalist first name for a child. Exploiting only within-family variation over time allows us to deal with the second challenge. That means we look at the decision of parents that have newborns children before and after the cities became part of Prussia in 1815 and compare the differences in their decision to a group of control cities that did not became part of Prussia. Using this empirical approach shows strong positive effects of the early attempts of nation-building.

The second chapter, "**When autocrats fail: Bismarck's struggle against the socialists**", analyses Bismarck's policies of carrot and stick against the socialist party. Famously, Bismarck introduced anti-socialist laws and social insurance to reduce the support for the socialist party. This narrative of *Zuckerbrot und Peitsche* (carrot and stick) is relatively uncontested in the historiography, explaining the origins of one of the first welfare states in history with a political maneuver.[4] However, few have asked whether Bismarck was successful.[5] The main challenge is to disentangle the general role of blue-collar workers, which formed the core of voters for the socialist party, from the specific effect of the social insurance, for which only blue-collar workers were eligible. To do so, I exploit the fact that local and industry-specific insurance schemes existed already before the introduction of Bismarck's public scheme. This variation allows me

[3] See, for instance, Sheehan (1993) and Planert (2002).
[4] Pflanze (1982) provides an overview on the different interpretations of Bismarck's strategies.
[5] While, for example, Wehler (2006, p.915) emphasizes the long-term integration of the workforce, Ritter (1983, p.50) sees social security as a failure in terms of its political goals.

to calculate the share of ex-ante insured workers at a county level. Building on this variation in the share of newly insured workers, I use a difference-in-differences and shift-share approach, while always controlling for the share of blue-collar. Contrary to what one (and Bismarck for that matter) would expect, the results consistently suggest that Bismarck was not successful with his policies of *Zuckerbrot und Peitsche* to reduce the electoral support for the socialist party. To rationalize this paradoxical result – the success of the socialist party after the introduction of social insurance – I argue that the socialist party used one loophole in the implementation of the public health insurance, namely voluntary health funds. These funds became attractive for workers despite higher monetary costs in comparison to other forms of health insurance and allowed political organization in times of otherwise repressive politics.

The third chapter, "**Migrating from the grain invasion: Trade shocks, labor markets and political polarization in Imperial Germany, 1880-1913**" (joint with Richard Bräuer and Wolf-Fabian Hungerland) explores the role of trade shocks on labor markets and electoral outcomes during the first globalization. One prominent narrative links the decline of agricultural areas driven by international pressure to the rise of more radical nationalist and anti-liberal forces in Germany, especially at the end of the 1870s.[6] Similarly, today many scholars argue that globalization shocks lead to more support for nationalist parties. This chapter partially confirms these patterns: The trade shock of the first globalization, the so-called "grain invasion" (O'Rourke, 1997), led to substantial migration and income effects. However, we do not see a corresponding effect on income per capita and political outcomes. We combine national-level trade data with newly digitized data on cultivation areas, income tax statistics, and employment at the county-level. We focus on the trade shock due to the "grain invasion" from the Americas, which started in the 1890s. Methodologically, we follow the recent literature pioneered by Autor *et al.* (2013) and rely on stacked first-difference regressions combined with instrumental variables. We attribute the surprising findings, which contrast the experience from the second globalization, to the higher level of migration as a crucial adjustment mechanism. Workers in counties hit by the shock in agriculture moved to the booming industrial centers and found many jobs that did not require a lot of specific human capital.

In the fourth chapter, "**Weber revisited: The Protestant Ethic and the Spirit of Nationalism**", Iris Wohnsiedler, Nikolaus Wolf, and I show the importance of national identity for understanding and testing one of the most prominent hypothesis in social science, namely Max Weber's Protestant Ethic. Following the recent sociological literature on Max Weber, we argue that Weber's nationalist agenda is crucial for the interpretation of the Protestant Ethic.[7] His study is not merely an analysis on the origins of capitalism, but also an attempt of political education. Specifically, Weber demanded that the political elites in Germany should do more against his fear (shared by many contemporaries) of a "Polanization" of the eastern parts of

[6] See Torp (2010) for an overview on this debate and Lehmann (2010) for a quantitative analysis. Torp (2010) objects to a purely economic explanation with a focus on falling prices, arguing that they did not fall noticeably until the 1880s.

[7] See, for instance, the work by Barbalet (2008) and Zimmerman (2006).

Prussia. Weber's writings (and activities as a member of various nationalist organizations), and the vast historical literature on German discrimination against the Polish-speaking minority make it necessary that any empirical investigation in the context of 19th century Germany on potential economic differences between religious denominations should also consider this ethnic conflict. Bringing this context to the data by controlling for the population share of the Polish minority, we show empirically that Protestantism does not have a significant effect on savings, literacy rates, or income levels across Prussian counties after 1870. Instead, there are, indeed, substantial differences between ethnic groups, likely due to ethnic discrimination. The main task here is to make sure that the empirical analysis integrates the context of these ethnic conflicts and its overlap with religious denominations. Existing empirical studies, most notably Becker and Woessmann (2009), do not consider these circumstances. Even more worrisome, their instrumental variable for the spread of Protestantism, distance to Wittenberg, is highly correlated with the share of the Polish-speaking population. Thereby, this instrument fails to disentangle the potential effects of Protestantism from the impact of ethnicity. To circumvent this issue, we use a different IV-approach following Spenkuch (2017) and rely on a previously unused cross-table on literacy by religious denomination. Both, the causal and descriptive evidence, show no differences between Protestants and Catholics, but between the German-speaking majority and Polish-speaking minority. We put together some suggestive evidence that these differences are due to German discrimination against the Polish minority, but more research is needed to prove the causality of this claim.

While covering quite different contexts in 19th century Germany, the four chapters, above all, are united in dealing with various aspects of social identity – either exploring potential causes of changes in social identities (chapter 1, 2, and 3) or possible consequences of social identity (chapter 4). They show that attempts of political elites to influence social identities in their interest can be successful (chapter 1) or backfire (chapter 2). Concretely, the first chapter contributes by providing causal individual-level evidence on identity changes in early 19th century with a methodological approach also applicable to other cases. The second chapter contributes to a better understanding of the effectiveness of carrot and stick policies by analyzing arguably the main historical case in which a government used these policies. Moreover, explanations on reasons of identity change that work for the present period do not transfer to the other periods (chapter 3). The idea that trade shocks lead to political polarization crucially depends on the level of labor migration and availability of alternative jobs. Thereby, the core contribution of chapter 3 is to show that labor markets in late 19th century were able to adjust to globalization shocks through migration. Finally, national identity and historical context should be taken into account when trying to understand and test famous theories (chapter 4). By doing so, chapter 4 contributes by showing that the economic effects of Protestantism are – if any – very small and that, instead, ethnic conflict mattered in late 19th century Germany for regional economic outcomes.

Besides exploring the role of social identity, chapter 3 and 4 also provide lessons to better understand regional inequalities within Imperial Germany. More concretely, trade shocks in agriculture led to massive labor migration, but not to different trajectories in income per capita.

Thus, the first globalization fostered economic reallocation within Germany without, however, showing the negative political consequences we observe in the present. Chapter 4 documents that economic activity in Germany before WWI was not only shaped by a growing rural-urban divide (as also chapter 3 suggests), but also by fragmentation along ethnolinguistic lines.

The rest of this dissertation proceeds as follows: I present the four self-contained chapters in chronological order. A consolidated bibliography and the appendices for each chapter are shown at the end of this thesis.

Chapter 1

On the origins of national identity. German nation-building after Napoleon

with Nikolaus Wolf

How did political elites shape national identities? In this paper, we investigate the success of nation-building policies in early 19^{th} century Germany. To elicit changes in identity at the level of individuals we use data on first names given in over 40.000 families in German cities. Using changes in the Prussian territory as well as variation within the same families over time, we find that parents in cities treated by nation building policies responded by choosing national (rather than Prussian) first names for their children.

1.1 Introduction

Why are people willing to die for their country? Why would parents send their children to battle or sign war bonds even when the situation is hopeless? Social scientists at least since the writings of Durkheim, Veblen, and Weber have been fascinated with these questions. Modern approaches are based on the concept of "social identity", namely the idea that people value their membership in social groups, and that such valuations are interdependent and changing over time. National identity is a type of social identity that stands out because it seems to evoke extreme emotions and forms of behavior that only compare to religion. Moreover, national identity relies less than other types of social identity (such as gender or race) on physical categories and more on everyday cultural practises such as singing anthems, waving flags, or marching in parades. Next, historians have long argued that the formation of national identities in 19$^{\text{th}}$ century in many parts of Europe was about the creation of an altogether *new* type of social identity, the collective imagination of a national community (Anderson, 1983).

But when and why did people begin to do this? Were early policies to strengthen this new type of social identity successful? Clearly, national identities were formed by many factors and over long periods of time. In this paper we focus on one particular period, and one particular mechanism, for which we provide causal evidence at the level of individuals. We investigate the success of nation-building policies in early 19$^{\text{th}}$ century Germany when the Prussian elites started to propagate the idea of a "German nation". To test for the effect of their policies, we elicit changes in identity at the level of individuals by using data on first names given in over 40.000 families in German cities. Specifically, we compare first names given to children in cities that – unanticipated (Clark, 2007) – became Prussian in 1815 after the Peace of Vienna to names in cities that stayed outside Prussia. Using these territorial changes as well as variation within the same families over time, we find that parents in cities treated by nation-building policies responded by choosing more national first names for their children. The same parents that neither had a national family tradition themselves nor had previously given their children German names, were much more likely to do so after becoming a Prussian subject. Crucially, we can use different connotations (Prussian vs German) of first names to provide evidence that it was indeed the strengthening of a new German national identity.

Our setting – Germany in the early 19$^{\text{th}}$ century – is attractive for a study on identity change for two main reasons. The formation of a German national identity preceded the establishment of a territorial nation state by several decades (Schulze, 1985, p. 58ff.) and there is an abundance of local data sources. Hence, we have variation in terms of territories over which elite policies mattered, and local data on their effects that we can use for causal analysis. This fact allows us to construct plausible control groups, which is more difficult for other countries where the territorial state coincides with the later nation state. In this context, a German national movement existed since the last third of the 18$^{\text{th}}$ century, carried by a new middle-class. But, it remained limited to small, intellectual circles. Only after 1813, when the Prussian elites started to appeal openly to national sentiments, did the movement begin to spread to larger parts of society. In 1815,

the Prussian elites started to adopt national identity policies to regain legitimacy and integrate the Rhineland and Westphalia into their enlarged territory. They enhanced ideas of a German nation by using speeches, symbols and theatre performances. Thus, our setting features two compelling aspects to investigate early nation-building policies: first, it allows to build treatment and control groups. Second, we capture a period in which the idea of a national identity and a "German nation" started to flourish.

Why would first names be a good indicator to capture changes in social, in particular national, identity? The major challenge for any study on social identity is empirical measurement. Identity can be seen as a latent variable, which in a historical context cannot be elicited by opinion polls or lab experiments. Also, there is little evidence to be gained from voting behavior as the political franchise was very limited and voting infrequent. Hence, we have to find a way to capture identities indirectly, via choices that people made at their time. These choices should be observable at the individual level, they should have an obvious relation to identity, be unsolicited and frequent. Following earlier suggestions from historical sociology, notably Gleitze (1962), Lieberson and Bell (1992), Gerhards (1997), and Wolffsohn and Brechenmacher (1999), and recent research on migration in economic history from Abramitzky *et al.* (2020), Fouka (2020), and Knudsen (2019) we elicit social identities and their changes over time by the type of first names that parents gave their children. We classify names based on the philological five-volume compendium on German first names by Seibicke (1996, 1998, 2000, 2002, 2007) into German national, religious, European, ancient and ruler first names.

Such an approach relies on two main assumptions. First, we need to assume that the choice of given names reflects the value that parents place on membership in a specific social group. Second, the valuation of parents needs to be at least partly transmitted to their children, be it directly by the parents themselves via education or indirectly, via their social environment. Both assumptions find very broad support from the recent literature on first names that also uses first names to capture social identity (Fryer and Levitt, 2004; Head and Mayer, 2008; Cook *et al.*, 2014; Abramitzky *et al.*, 2020; Fouka, 2020). We provide further evidence by showing substantial differences between the share of national first names for the soldiers on loss lists and those who were awarded during the German-French War in 1870/71, also if we condition the analysis on different hierarchy levels.

Using first names for the empirical analysis, it is challenging to rule out various confounding factors that also might account for the name choices of parents, such as differences in education, social status or individual traditions of parents. This is a problem for modern data, but even more so in a historical setting with limited data on control variables. To deal with this, we systematically exclude all parents that had national names themselves. This is likely to bias our results downwards, as parents with a national family tradition would be more susceptible to national propaganda. For the remaining parents we use only variation in the type of name choice made by the same parents over time. Hence, we only need to assume that unobserved parent-level factors remain constant as long as the mother was in child-bearing age.

Our study is related to the growing empirical literature on nation-building. Studies have highlighted the positive impact of, for instance, schooling (Bandiera *et al.*, 2019), intergroup contact (Bazzi *et al.*, 2019), welfare-provision (Caprettini and Voth, 2020) or external enemies (Dell and Querubin, 2018) for nation-building along various dimensions. Nation-building policies can also backfire: Fouka (2020) investigates forced assimilation policies in several US states that targeted the German minority and led to less integration of this group. Exploiting the quasi-exogenous division of the French regions Alsace and Lorraine in 1870/71, Dehdari and Gehring (2019) show that repressive policies of the nation state can strengthen regional identities. More broadly, our paper is related to studies on the determinants of identity formation and change, for instance, Battu and Zenou (2010) as well as Manning and Roy (2010). However, causal evidence on the determinants of changes of identity formation is scarce. Notable exceptions are the aforementioned study by Fouka (2020) for 20$^{\text{th}}$ century US and Clots-Figueras and Masella (2013), who shows that allowing bilingual education strengthens Catalan identity. We add to these empirical strands of literature evidence on the origin and spread of a "national identity" for the early 19$^{\text{th}}$ century, when arguably for the first time in history national identity became a mass phenomenon. Moreover, we build upon existing identification strategies by capturing changes within families. Thereby, we provide evidence on identity changes at the individual level as it occurred more than 200 years ago.

Also, our paper is related to theoretical approaches in economics that aim to explain the formation of social identities. A common starting point is that individuals value their perceived distance from specific group prototypes, as suggested by Akerlof and Kranton (2000). Building on this, several authors have suggested mechanisms to account for changes in social identities.[8] The work by Alesina *et al.* (2017, 2019) is most relevant for this study: their idea is to distinguish between an "elite" – a small group of agents with exogenous preferences – and a larger group of agents with endogenous preferences. The elite can use policies and institutions such as pamphlets, state celebrations or schools to shape the preferences of the population. We contribute to this literature by accounting for the historical development of national identity, which was rather about the creation of an altogether new type of social identity, the collective imagination of a national community (Anderson, 1983), than about homogenization policies. Our research suggests that current theories on nation-building are incomplete at best.

The rest of our paper is organized as follows: we introduce the historical background and our data in section 1.2. Section 1.3 contains our empirical strategy. In section 1.4, we present our results on the treatment of 1815 for between and within family variation, including various robustness checks. Section 1.5 concludes.

[8] Other approaches consider technological and economic change as a driver of identity change. The social anthropologist Gellner (1983) argues that industrialization, accompanied by large-scale migration from villages to cities and the need for standardization devalued old regional identities and contributed to the rise of a new broader "national identity". Related, Shayo (2009) develops a model, where group status depends among other things on relative income and perceived distances from group prototypes. Here, income growth and improved communication can lead to the spread of a "national identity", first among the middle-class and later among the poor. Also related is the large literature on the transmission of culture (Bisin and Verdier, 2001).

1.2 Historical background, data, and validation

Our empirical study is focused on the years when, for the first time, the German national movement began to spread beyond small intellectual circles. To understand the historical context we will describe the political situation at the time. Next we will discuss why the Prussian state had temporarily a strategic interest to appeal to national sentiment, and what policies were used to shape identities. After presenting the historical background, we explain our measurement of national identity.

National identity in early 19th century Germany

The political situation around 1815

In the last third of the 18th century, the Holy Roman Empire with the Emperor in Vienna still existed, but it was little more than an empty shell. The German lands were fragmented into several hundred territories ruled by various competing dynasties, church territories and free city states. The two leading powers were Habsburg and Prussia, both comprising large and heterogeneous territories outside the Empire, and both striving for a more or less enlightened type of absolutist state. At the same time, the old feudal order, with a division of power between the ruler and the estates was being replaced by a centralization of power, with an absolutist ruler supported by a modernized state bureaucracy. This centralization of political power was, according to Schulze (1985, p. 240), accompanied in many societies in Europe by a deep crisis of loyalty, where political, economic and cultural change destabilized old identities. The dramatic events in America 1776 and France 1789 intensified this loyalty crisis and were initially greeted by many intellectuals on the continent as the beginning of a new era. However, the initial enthusiasm quickly gave way to anti-French feelings. The occupation of large parts of Germany did not only foster modernization, e.g. by eliminating many small states, but also led to large and growing burden in terms of taxes and conscription for the war. Especially the self-coronation of Napoleon as Emperor in 1804 and the humiliating defeat of Prussia in 1806 sparked a new type of German national sentiment, that was geared against the French occupation.

Around the same time in Berlin formed a patriotic circle that included the publisher Georg Andreas Reimer, as well as Ernst Moritz Arndt, Friedrich Jahn, Friedrich Schleiermacher and the Prusssian officer August von Gneisenau (Bartmuß*et al.*, 2008). Notably Jahn became prominent as founder of the German gymnastics movement (*Turnbewegung*) in 1811, which shortly spread to other German states and became the organizational backbone of the early national movement (Düding, 1984). Ernst Moritz Arndt wrote very popular pamphlets and poems such as "Was ist des deutschen Vaterland?" (1814), which claimed all territory where German was spoken for a new German national state. But this early national movement remained very small, not least because the authorities in the various German states hesitated to support it. Some states, like Prussia in 1806, had lost not only their independence and military power but also large parts

of their territory, while others like Bavaria had been compensated for a loss of independence by territorial gains and upgraded status.[9] Moreover, the aristocracy all over Europe feared the liberal and revolutionary tendency among the national movements, as they could easily turn against their privileges, wealth, and indeed their life.

Prussias strategic interest

Within this tension, a group of reformers among the Prussian elite realized that the national and anti-French sentiment could be used to the benefit of the Prussian state. A key motive was the mobilization of people for a levee en masse against the occupation (although they were eager to avoid this term). Already in 1797, Gerhard Scharnhorst, then officer in Hanover, observed that the French army had an advantage due to the messianic fighting spirit of the French soldiers (Dörner, 1995, p. 112). After the humiliating peace of Tilsit in 1807 he was promoted to major-general and led, together with his disciples von Clausewitz and von Gneisenau, the effort of a fundamental reform of the Prussian army. He introduced new recruitment systems to circumvent the limitations imposed on Prussia since 1807 and strengthened the ties between army and population, e.g., by opening the career path of officers to non-noble families.

After Napoleons defeat in Russia in late 1812, Scharnhorst, Gneisenau, Stein and other reformers urged the reluctant king of Prussia, Frederick William III to side with Russia and finally appeal to patriotism and the national movement. As a first step towards this, in February 1813 the Prussian government appealed to form voluntary corps and decreed that every man above 20 had to wear in public the national cockade as a sign of their patriotism (Hagemann, 2019, p.165). It was only after Prussia had declared war to France in March 1813 that the king decided to made a proclamation "To my people". With this he appealed for the very first time to national pride of "Germans and Prussians" to fight-off the foreign occupation. This proclamation was published as a pamphlet and as an article in the leading newspaper of Breslau (today Wroclaw, where the king resided in 1813), and sent to all postal offices of Prussia at the time. Also in March 1813 the king established the Iron Cross, a new military decoration to be awarded for bravery without regard to rank nor social status.

The battle of Leipzig in October 1813 brought the military success over France that Prussia had hoped for, but the monarchy still had to face major challenges. At the international peace congress in Vienna 1814/15 Prussia had aimed for an annexation of the Kingdom of Saxony. Instead, due to a British intervention, Prussia gained large territories in the West (the Rhineland and Westfalia), which were disconnected from the Prussian mainland. In the words of Christopher Clark: "Berlin failed to get what it wanted and got what it did not want [...] The creation of a large Western wedge along the river Rhine was a British, not a Prussian, idea" (Clark, 2007, p.389). The new territories had a predominantly catholic and often urban population (Huning

[9] With French support, the Prince-elector Maximilian was crowned King Maximilian I. of Bavaria in January 1806.

and Wolf, 2019). This stood in contrast to the more rural and protestant Prussian mainlands and the protestant monarch Frederick William III himself. The main cities in the new territories like Cologne, Muenster, or Aachen had very different traditions as either free cities or catholic Prince-Bishoprics.

National identity policies

Hence, after the new territories had been formally incorporated into the Prussian state in 1815, the authorities faced the challenge to establish the legitimacy of their rule and win the support of their new population. A main instrument for this were royal proclamations and the official homage ceremonies, which took place in 1815. Proclamations and homages were traditional tools of government since the middle ages. But crucially for our study, in 1815 they had an entirely new feature: the attempt to create a new narrative with the king of Prussia leading the German nation. In the proclamation of 5 April 1815 to the inhabitants of the "Rhine-lands unified with the Prussian monarchy" the king Frederick Wilhelm III stated that at the congress of Vienna he agreed to the burden of defending the "endangered borderlands" out of respect for the "unified German fatherland" (cited after Kotulla, 2010, p.591). Moreover, he gave the people of the Rhineland back to their "German fatherland and to an old German princely family" (cited after Kotulla, 2010, p.591), and promised to honor their religion.

In this spirit, the Prussian authorities organized homage ceremonies in the newly gained territories. The ceremony for the Rhineland took place in Aachen on 15 May 1815, the ceremony for Westphalia in Muenster on 18 October 1815. The decision for Aachen was a reference to the former residence of Charlemagne. As General Gneisenau and Governor Sack stated in their official announcement, the city was chosen because "only the city of Aachen unites age, grandeur and suitable local with the dignity of a coronation city for the most elevated German rulers, where after Charlemagne no less than 35 German Emperors were crowned" (cited after Tschacher, 2010, p.259). The date in turn coincided with Pentecost, which had been abolished under French rule but was now being reintroduced as a religious holiday. The choice of date for the second ceremony in Muenster was a very explicit reference to national identity: it coincided with the second anniversary of the battle of Leipzig, which already then played a pivotal role for the collective memory, mainly due to the publications by Ernst Moritz Arndt and Friedrich Jahn (Hagemann, 2002, p.481f). The orchestration for both ceremonies was similar: they were extended over several days and included the celebration of a mass in the cathedral, processions, fireworks, official banquets, feeding of the poor and theatre performances. Importantly, in both cases the authorities had invited representatives of all parts of society, including the nobility, clergy, burghers and peasants. For Aachen, we know that there was also a common dress-code, where all civilians were obliged to wear black dress and the national cockade (Tschacher, 2010, p.259). Here, the play "Deutsche Treue" by August Klingemann was performed, which placed the unification of the Rhineland with Prussia in the context of the reconciliation between Frederick the Fair of Austria and Louis IV (the Bavarian) in 1325 after their struggle for the German

crown. Afterwards, the celebrations were popularized in a flood of pamphlets and newspaper articles, most prominently a serial written by Ernst Moritz Arndt (Tschacher, 2010, p.267).

The celebration in Muenster a few months later was quite similar, but here the association between the king of Prussia and a new German identity was made even more explicit. The festivities took place over several days, they were similar to those in Aachen, elaborate and rather expensive (Lahrkamp, 1976, p.128). The chosen theater play "Der Altar im Walde" made a clear reference to the idea of a "German nation" (Schwengelbeck, 2007, p.142ff). In this play, the king of Prussia was presented as the new Hermann, the legendary chieftain of the Germanic Cherusci tribe who fought the Roman invasion. The topic had been popularized as a national allegory since Arndt asked in 1805 for a "new Hermann" to fight Napoleon and Heinrich von Kleist's drama "Die Hermannsschlacht" (1808). Now in Muenster 1815, king Friedrich Wilhelm III himself was depicted as the new Hermann, saviour of the German nation. The author of the play was Wilhelm Aschenberg, a lutheran pastor and writer from the nearby city of Hagen. In 1814 Aschenberg had founded the journal "Hermann" that propagated ideas of a pro-Prussian German nationalism, along the lines that would suit the Prussian government (at the time). As in the case of Aachen, the celebration was followed by several articles in newspapers and special publications to spread the word.

The Prussian authorities clearly attempted to create a new narrative to show the king of Prussia as the leader of the *German* nation, particularly in the new provinces. Their intention was purely strategic, with the aim to use the broader idea of a German nation to turn the new population into loyal subjects of the king. And indeed, once the new territorial order was established, the Prussian authorities tried to silence the national movement again (Echternkamp, 1998, p. 232). But to what extent did their strategy in 1815 succeed? How can we test if individuals that experienced these celebrations or had heard and read about these events, actually changed their identity, and behaviour?

Measuring national identity in early 19th century Germany

We want to elicit national identity and its changes over time by the type of first names that parents gave their children. To this end, we collected data from births registers for eight German cities based on city and church archives and several genealogy websites, which we typed in.[10] Thereby, we construct a sample with data on more than 40.000 parents and their children. For these births, we have the following information: first and last name for children and their parents, as well as place and year of birth of the children.

To capture the changes in the exposure to identity policies described above, we include cities from three different groups in our sample. First, cities that became part of Prussia after the

[10] Our sources are Bauernfeind (2009) for the city of Nuremberg, Bistumsarchiv Münster (2020) for the city of Muenster, and The Church of Jesus Christ of Latter-Day Saints (2003) for all other cities.

Peace of Vienna in 1815 (Aachen and Muenster), i.e., treated cities. Second, cities that did not become part of Prussia in 1815, but only in 1866 (Frankfurt and Hanover) or Germany in 1871 (Heidelberg, Mannheim, and Nuremberg), i.e., control cities. Third, we include Berlin as the capital city of Prussia. Moreover, we focus on larger cities because our approach requires a sufficient number of families with variation in name choices over time.

Consider the two treated cities, Aachen and Muenster. Both were predominantly catholic cities, in difference to the protestant mainlands of Prussia. The city of Aachen had played an outstanding role in the history of the old Holy Roman Empire, as the place where the Emperor was crowned German king until 1531. The city was an Imperial free city until the French occupation in 1794, and became Prussian in 1815. In contrast to Muenster, Aachen was under direct French control between 1794 and 1814. The city of Muenster was capital of the prince-bishopric Muenster, the largest clerical territory in the HRE, until 1802 when it briefly became part of Prussia for four years. In 1806, Napoleon and his troops occupied Muenster. Prussian and Russian troops drove the French troops out of Muenster in 1813, and the city became officially part of Prussia after the Congress of Vienna in 1815. Note that our control group cities also experienced territorial change during our period. Hence, the experience of territorial change as such will not explain our findings.

To capture changes in national identity in our sample, we need to classify the first names. Here, we rely on a five volume encyclopedia on German first names from Seibicke (1996, 1998, 2000, 2002, 2007). With this, we capture around 95% of all entries in our sample. We differentiate between national, religious, European and ancient first names. Furthermore, we account for family. We count those first names as Germanic whose origin lies in old high German (*althochdeutsch*) or germanic (*germanisch*) language, according to Seibicke. We cross check our list of national first names with a book from Khull (1909) that aimed to increase the consciousness for Germanic first names in order make sure that the names on our list had a nationalist connotation in the 19$^{\text{th}}$ century.[11]

Before showing the development of national first names, we need to discuss the case of Aachen. In this case, the political changes themselves led to changes in the way first names were recorded. It is likely that the French bureaucracy "francicised" many German names, although we have no direct evidence on this (Kramer, 1993, p.225). In the case of the parents' first names, this is not problematic as it is unlikely that the parents were given French names in late 18$^{\text{th}}$ century Aachen before the French occupation. Thus, we use the German translation of the French first names (e.g. Guillaume/Wilhelm). For the children, however, this becomes more complicated because we do not know whether parents adjusted to the new rulers by choosing French first names or whether instead the French administration "francicised" the German names. Such adjustments are potentially important for our results. To deal with this, we use two radically different versions of the data for Aachen: an unadjusted version, where we assume that all

[11] Among the most popular national first names in our period are Carl, Friedrich, Heinrich, Wilhelm, Ludwig, and Ernst for boys and Caroline, Wilhelmine, Friederike, Ida, Albertine, and Bertha for girls.

French versions of German names reflect indeed the choice of parents. With this we might substantially underestimate the share of Germanic first names for the pre-treatment period and thus potentially overestimate our treatment effect. As an alternative to this, we adopt a most "conservative" interpretation of the data for Aachen, assuming that the French administration systematically "francicised" all German first names, also against the wish of the parents. Hence, we adjust the data and classify all French versions of Germanic names as national names. We lack direct evidence on this, but if anything this introduces a strong bias against us finding a treatment effect.[12] Unless stated otherwise, all our results are based on this adjusted version of the data for Aachen Note that we do not face a similar problem for families in the city of Muenster.

In Figure 1.1, we apply the classification to our data and summarize the share of national first names by our two groups to give an overview of the trends in our sample for the early 19$^{\text{th}}$ century. We see that the share of national first names remain relatively stable over the whole period in our control group (with a small increase in 1813), while we observe an increase for our treatment group after 1815 from around 13% to around 18%. If we drop the conservative adjustment for Aachen, the increase is even greater (from 10% to 18%). We also show how the share of Friedrich and Wilhelm (and their female variations) developed over time in treatment and control cities. Interestingly, we do not see an increase in their share after Prussia took over the Rhineland and Westphalia.

Table 1.1 provides descriptive statistics by city. Overall, it confirms the evidence from Figure 1.1. With the exception of Hanover, the average share of national first names before and after the treatment remains stable in our control group. At the same time, we clearly see an upward trend in our "treated" cities. We also see that our adjustment for Aachen makes a major difference and will bias the results against us finding any effect.

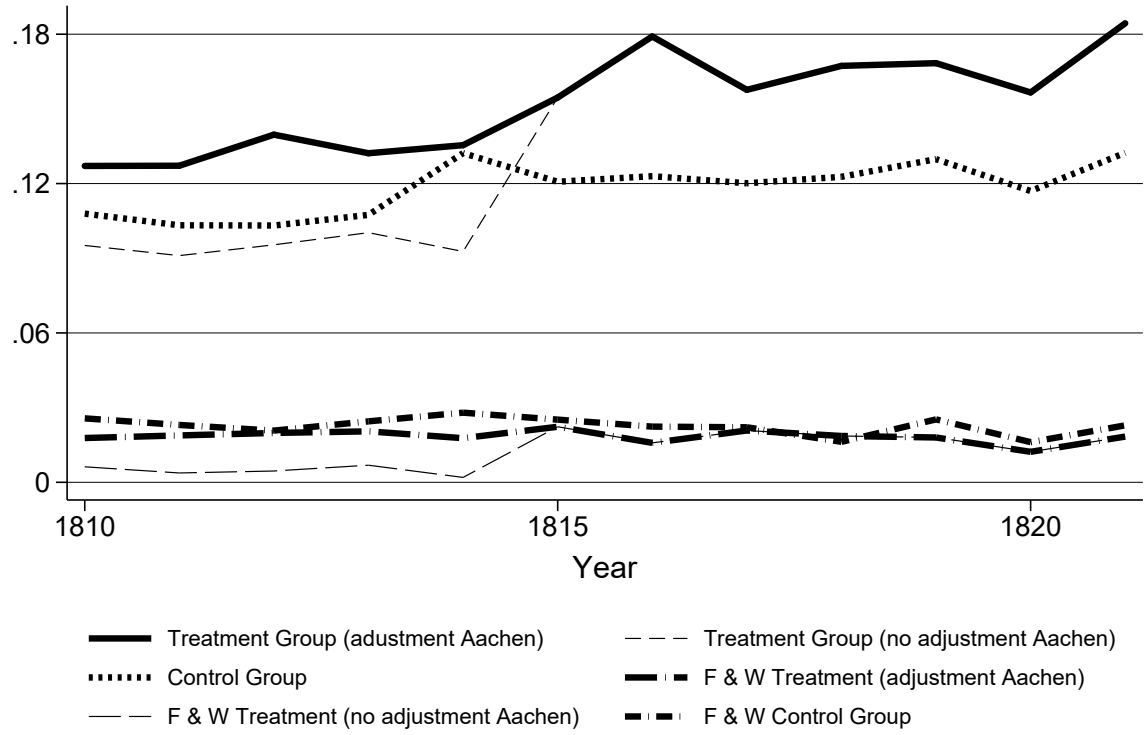
Validation

Can changes in first names really capture identity change? We need to assume that the choice of given names reflects the value that parents place on membership in a specific social group. And second, the valuation of parents needs to be at least partly transmitted to their children, be it directly by the parents themselves via education or indirectly, via their social environment. Both assumptions find very broad support from the recent literature on first names that also uses first names to capture social identity (Fryer and Levitt, 2004; Head and Mayer, 2008; Cook *et al.*, 2014; Abramitzky *et al.*, 2020; Fouka, 2020).

To validate these assumption in the context of 19$^{\text{th}}$ century Germany, we collect data from loss lists and lists with honored soldiers for the German-French War in 1870/71 (Verein für

[12] In fact, we do not observe a break in our data once the French administration left Aachen in January 1814, which might suggest that the unadjusted version is not too far off.

FIGURE 1.1
SHARE NATIONAL FIRST NAMES, 1810-1821



Note: The plot captures the share of national first names and of Friedrich/Friederike and Wilhelm/Wilhelmine by treatment and control group conditional on family tradition. Treated cities: Aachen and Muenster. Control cities: Frankfurt (Main), Hanover, Heidelberg, Nuremberg, and Mannheim.

TABLE 1.1
DESCRIPTIVE STATISTICS

City	Before 1815	After 1814	N per year
Aachen (adjusted)	13.69	15.63	854.56
Aachen (unadjusted)	6.24	15.63	854.56
Muenster	12.73	17.72	459.89
Frankfurt	13.30	13.81	1059.70
Hanover	19.90	23.34	595.72
Heidelberg	6.62	7.83	1442.68
Mannheim	5.95	6.62	461.58
Nuernberg	6.10	7.22	913.47
Berlin	30.44	30.81	2564.64

Notes: Share national first names (in %) by city and before/after treatment.

Computergenealogie, 2014). The loss lists include information about the first and last name, the location, the regiment, the year the soldier was listed on the loss list, and contain around 160.000 entries for the German-French War in 1870/71. The loss lists indicate not only participation in the war, but active participation, as they refer to soldiers that were either killed, wounded, captured or went missing. However, none of these events needs to reflect voluntary behavior of soldiers.

In order to capture voluntary engagement during wars, we use lists with honored soldiers. We digitize the names of all 1295 soldiers honored with the *Eiserne Kreuz* during the German-French War from 1870/71 (Königliche General-Ordens-Kommission, 1878) and categorize them based on four status groups. The basic idea here is that the distinction of soldiers for bravery should be a much better indicator for voluntary behavior than active war participation as reflected on the loss list. We might expect that soldiers with a strong national(ist) identity would be more willing to fight and hence more likely to receive the Iron Cross than others. If Germanic first names would capture national identity, we expect to find a strong positive correlation between national names and honors.

While this allows us to link first name choices made by parents to the behavior of their children, such an approach has disadvantages. Unfortunately, we have no other control variables for the soldiers. However, we limit this problem by testing for differences within a hierarchy level. The military was a reflection of social classes, so that relatively similar social characteristics can be expected within one hierarchy level.

Comparing the loss lists and the honored soldiers in Table 1.2 shows overall substantial and statistically significant differences. More specifically, we find differences between the share of national first names for the soldiers on the loss lists and those who were awarded during the German-French War in 1870/71, also if we condition the analysis on different hierarchy levels. Note that the differences are very sizeable. The difference in terms of the share of national first names between *Offiziere* on loss lists and award lists is more than 30 percentage points. We find similarly large differences for other hierarchy levels. Analyzing more specific national first names, we also find differences. Interestingly, the relative difference is more pronounced for Ernst and Hermann – well-known nationalist first names – than for Friedrich and Wilhelm – names with overlapping connotations, as these were also the most prominent ruler first names of that time. Overall, the evidence on loss lists and war decorations from the war of 1870/71 supports our idea to capture identity changes using changes in first names.

1.3 Empirical strategy

In order to determine the causal effect of Prussian policies in 1815 on the formation of national identities, we use evidence on first names and exploit variation between and within cities and families. Therefore, we compare decisions by parents in our *treated* families in Aachen (in the

TABLE 1.2
NATIONAL FIRST NAMES, WAR PARTICIPATION, AND WAR DECORATION

	Share national first names...		
	...Loss Lists	...Honored Soldiers	t-statistic
National First Names	0.309	0.553	17.739***
... General (*General*)	0.190	0.604	14.106***
... Officer (*Offiziere*)	0.200	0.532	20.503***
... Sergeant (*Unteroffiziere*)	0.333	0.579	5.864***
... Other soldiers (*Mannschaft*)	0.314	0.625	2.489**
Friedrich and Wilhelm	0.109	0.140	3.243***
Ernst and Hermann	0.029	0.057	4.297***

Notes: * p<0.1, ** p<0.05, *** p<0.01.
Sources: Verein für Computergenealogie (2014) and Königliche General-Ordens-Kommission (1878).

Rhineprovince) and Muenster (in Westphalia) and our *control* families in Frankfurt, Hanover, Heidelberg, Mannheim, and Nuremberg, i.e., cities that were not part of Prussia. Unless stated otherwise, we always use the adjusted data for Aachen.

However, just comparing the frequency of name choices in treated versus non-treated cities could be misleading. There is a host of confounding factors that might also account for the name choices of parents, such as differences in education, social status or individual traditions of parents. In a setting where we compare parents in treated cities to parents in a control group of untreated cities, we need to take this heterogeneity within cities into account. We address these problems in two steps. First, we systematically exclude all parents that had national names themselves, because their decision in favor of a national first name could also reflect existing family traditions and such parents might have been more susceptible to national propaganda. Second, and more importantly, we use family-fixed effects: we also estimate specifications, where we use for the remaining parents only *variation* in the type of name choice made by the same parents over time. However, this is only possible for families who get at least one child before and after the treatment.

Using within–family variation has several main advantages. This strategy allows us to control for all time–invariant family characteristics. Moreover, we only compare families in similar age groups as we are only interested in those families that get children in the years before and after the specific year. And we can rule out that migration into Prussian cities drive our results because we only consider families who already live in the respective city before the treatment year and stay there after the treatment. This controls for example for the families of Prussian officers moving into Aachen or Muenster after 1815.

We use the following differences-in-differences estimation employing an OLS model as our main specification:

$$NationalName_{fct} = \alpha_f + \beta(TreatedCity_c \cdot Post1815) + \gamma Post1815 + \epsilon_{fct}, \qquad (1.1)$$

where α_f are family fixed effects and $Post1815$ equals to 1 for the post-treatment period and 0 otherwise. $NationalName_{fct}$ is a dummy variable that equals one if at least one child in family f has a national first name in city c in one period. Note that we collapse our sample in a pre- and post-treatment period in our main specification. Thereby, we control not only for serial auto-correlation following Bertrand and Mullainathan (2004) but also take into account that a specification based on yearly within family variation dramatically restricts our sample size. The coefficient of interest is β that indicates the treatment effects on parents' name choices by comparing the average change in national first names before and after for our treatment group, compared to the average change befor and after for our control group.

As second specification, we use a flexible difference-in-differences model, also to control for potential pre-trends:

$$NationalName_{fct} = \alpha_c + \theta_t + \sum_{t=1810}^{1821} \beta_t (TreatedCity_c \cdot Year_t) + \epsilon_{fct} \tag{1.2}$$

The coefficients of interest is β_t that indicates the effect of living in a treated city in $Year_t$. With this specification, we control for time invariant city characteristics by including city fixed effects (α_c) as well as more general time trends. θ_t are year fixed effects.

The identifying assumption in this setting implies that the decisions by parents regarding first names in cities that become part of Prussia and in cities that do not become part of Prussia would follow the same trend in absence of the treatment. We will show that this is indeed the case for our treatment analysis. Given the course of events after the defeat of Napoleon in 1813/14, and Prussia's negotiations at the congress of Vienna (Clark, 2007), the territorial change that occurred in 1815 was difficult to anticipate.

For our empirical analysis with family fixed effects, we need to construct a common family ID to trace children with the same parents over time. To improve matching, we follow a recent approach by Abramitzky *et al.* (2019a) and use the Jaro-Winkler distance. Appendix A.1 provides details on our procedure.

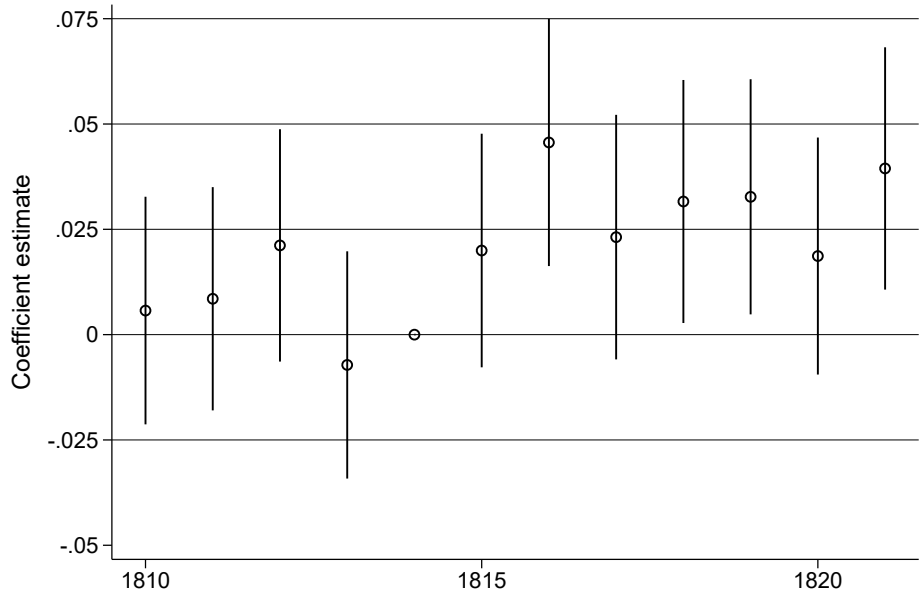
A potential concern for our analysis could be that conditioning on families who have a child before and after treatment (identified by our common family ID) may introduce some selection bias. For example, it would be worrying if in treatment cities the share of national first names was lower for observations with a common family ID than for those without common family ID before treatment and higher after treatment. To account for this issue, Table 1.3 presents the share of national first names before and after treatment (like in Table 1.1) and whether or not we found a match for parents before and after treatment. While there is some variation between cities, the overall pattern in Table 1.3 is reassuring and no clear selection visible. For the case of Muenster, the share of national first names is somewhat lower for "matched" families before treatment and higher after. However, the opposite applies to Aachen (smaller in magnitude). There are also cases in the control group where selection might work against our treatment

(e.g. Frankfurt, Nuremberg) as well as cases that could work slightly in favour of our treatment analysis (e.g. Hanover).

1.4 The effect of nation-building

Figure 1.2 presents evidence on the common trend assumption. The coefficients based on equation 1.2 for the pre-treatment period, with 1814 as reference year, are statistically insignificant (see also Table A1). Between 1811 and 1814, we see some variation in the point estimates, however, the estimates are not significant and no upward trend is visible. The coefficients after 1815 are substantially higher and in most cases significant using city fixed effects. Overall, this suggests that the common trend assumption holds for our estimation.[13]

FIGURE 1.2
EVENT STUDY, 1810-1821, CITY FE



Notes: The plot captures the coefficients from equation 1.2 based on column 1 from Table A1. Reference year: 1814. Confidence level at the 90%-level

The results in Table 1.4 provide evidence for overall strong positive treatment effects of national identity policies. In panel 1 we include all national first names. We see a significant effect using city fixed effects (column 1). The effect size increases in column 3, once we rely on our preferred specification with family fixed effects, but the effect becomes insignificant.[14] This higher point

[13] Using family fixed effects and estimates per year we also observe insignificant point estimates without an upward trend, and increasing point estimates after 1814. However, with family effects the yearly estimates are not significant, likely due to the very small sample size per year. Note that such a specification requires families to have children every year, which, of course, is not often the case. The small sample size per year is the main reason why we prefer to collapse the sample in pre- and post-treatment.
[14] Note that, once we use family fixed effects, the number of observations is an order of magnitude smaller: 4822 compared to 46577.

TABLE 1.3
NATIONAL FIRST NAMES BY SELECTION INTO FAMILY

	Family with child before and after (Match)			Family with child before or after (No Match)		
	N	Before	After	N	Before	After
Aachen	5385	13.80	15.25	4784	13.55	15.96
Muenster	2450	12.09	18.80	2981	13.37	16.96
Frankfurt	6076	13.50	14.37	6232	13.11	13.32
Hanover	2633	19.19	22.42	4212	21.57	24.07
Heidelberg	10331	6.27	7.22	6559	7.45	8.99
Mannheim	2915	6.60	6.83	2226	6.10	6.90
Nuremberg	3511	9.19	9.00	4248	7.56	9.64
Berlin	13741	31.46	32.37	15572	30.52	30.61

Notes: Share of national first names in % for observations with one child either before or after treatment and observations with at least one child before and after treatment.

estimate suggests that unobserved variation at the family level biases the results downwards. A possible explanation could be that social status affects the willingness of parents to chose new, innovative names. Once we control for this, families in a treated city are on average 7 percentage points more likely to give children a national first name after the treatment and comparing to the control group. Given the average share of national first names of around 15% in this period, this effect is meaningful. In a next step, we allow for heterogeneous effects for our treated cities, Aachen and Muenster. With the conservative adjustment for Aachen we do not find a significant effect, while we find a strong positive effect for Muenster (see column 2 and 4 in Panel 1 of Table 1.4). Again, the treatment effect for Muenster based on family fixed effects (column 4) is much larger than the effect based on city fixed effects (column 2).

In Panel 2 of Table 1.4 we exclude all children named Wilhelm, Wilhelmine, Friedrich, and Friederike after 1815. Thereby, we want to rule out a specific Prussian "ruler" effect. King Friedrich Wilhelm III was presented as king of the Prussians *and* Germans. But if the entire effect would be driven by naming more children Friedrich or Wilhelm (both names of germanic origin), we could not rule out that it was primarily Prussian homogenization policy. If, however, our effects were driven by national identity politics, we would expect to find larger treatment effects compared to panel 1. And indeed, the treatment effects increase, and become significant, even though we have a smaller number of observations. This holds notably once we focus on variation within families (column 4). The Prussian identity policies after 1815 did not induce parents to name their children more often after Prussian rulers, but made them chose other national first names more frequently (compare also Figure 1.1). Again, as in Panel 1, the treatment effects are larger using family fixed effects compared to city fixed effects.

TABLE 1.4
TREATMENT ANALYSIS 1815

Dep. var.: National First Name	City FE		Family FE	
	(1)	(2)	(3)	(4)
Panel 1: With Wilhelm and Friedrich				
Treated × Post1815	0.028***		0.070	
	(0.008)		(0.044)	
Post1815	0.009**	0.009**	0.128***	0.128***
	(0.004)	(0.004)	(0.024)	(0.024)
Aachen × Post1815		0.015		0.027
		(0.010)		(0.050)
Muenster × Post1815		0.052***		0.160**
		(0.013)		(0.067)
City FE	✓	✓		
Family FE			✓	✓
Observations	46577	46577	4822	4822
Families	38741	38741	2411	2411
R-squared	0.03	0.03	0.02	0.02
Panel 2: Excluding Wilhelm and Friedrich				
Treated × Post1815	0.028***		0.091**	
	(0.008)		(0.045)	
Post1815	-0.013***	-0.013***	0.031	0.031
	(0.004)	(0.004)	(0.025)	(0.025)
Aachen × Post1815		0.012		0.034
		(0.010)		(0.052)
Muenster × Post1815		0.058***		0.211***
		(0.013)		(0.069)
City FE	✓	✓		
Family FE			✓	✓
Observations	46007	46007	4530	4530
Families	38249	38249	2265	2265
R-squared	0.03	0.03	0.01	0.01

Notes: * p<0.1, ** p<0.05, *** p<0.01. We only include parents without national first given name. Clustered standard errors at the family level. Treated cities: Aachen and Muenster. Control cities: Frankfurt (Main), Hanover, Heidelberg, Nuremberg, and Mannheim. Results based on equation 1.1. Data is collapsed in pre-and post period.

After presenting our main result, we discuss several other dimensions of our setting. The identity policies in 1815 in the new provinces were location-specific as discussed in Section 1.2 above, and should have affected the centre less than the treated cities. Therefore, in Panel 1 of Table A2 we use families in Berlin as control group. With this, we find a strong treatment effect for families in both, Aachen and Muenster using city effects and still a strong effect using family effects for the case of Muenster.

In Panel 2 of Table A2, we drop the assumption that all first names were "francicised" before 1815, such that we need to adjust first names for this. Without any such adjustment, the effect for Aachen becomes much stronger and even surpasses the effect for Muenster. This result, of course, is based on quite strong assumptions in our favor. Presumably, the "correct" effect for Aachen lies between the two results, and hence is similar to our finding for Muenster. In Panel 3 of Table A2 we use a Logit estimation to account for the fact that our dependent variable is binary. The results remain qualitatively similar.

As last step of our analysis, we consider the territorial expansion of Prussia in 1866, when Frankfurt and Hanover became part of the Prussian state. This example provides a insightful validation exercise. In difference to 1815, this enlargement of Prussia in 1866 did not have the connotation of German unification, but rather of Prussian power politics geared against the idea of a German national identity, due to the German-Austrian War, or "Fraternal War" (Schulze, 1985, p.235f). Here, the incorporation of the new population was not accompanied by a similar national propaganda as in 1815, so we would not expect to find an effect for the treated cities in 1866. Indeed, as shown in Table 1.5, we do not find similar positive treatment effects for 1866 when Frankfurt and Hanover became part of Prussia. We even see negative and significant treatment effects for Hanover. However, we see that the national idea (as captured by national first names) was generally spreading after 1866, but unrelated to Prussia's territorial gain. This is a reminder that nation-building by elites was only one factor behind the rise of national identities, besides many others.

1.5 Conclusion

In this paper we investigated the effect of early nation-building policies on social identities. To elicit individual-level changes in identity we use data on first names given in German cities. We find an overall positive and significant treatment effect as documented in Table 1.4. Notably, for a sub-sample of our data we are able to exploit variation *within* families over time, after controlling for family fixed effects. We interpret this as evidence that national identity policies can indeed change individual identities. Furthermore, we distinguish between specific Prussian names and national names and found that parents in treated cities responded by choosing national, not Prussian first names for their children.

TABLE 1.5
TREATMENT ANALYSIS 1866, OLS

Dep. var.: National First Name	City FE		Family FE	
	(1)	(2)	(3)	(4)
Treated × Post1866	-0.003		-0.049*	
	(0.008)		(0.030)	
Post1866	0.008*	0.008*	0.066***	0.066***
	(0.004)	(0.004)	(0.018)	(0.018)
Hannover × Post1866		-0.008		-0.118***
		(0.011)		(0.038)
Frankfurt × Post1866		0.002		0.019
		(0.009)		(0.038)
City FE	✓	✓		
Family FE			✓	✓
Observations	61786	61786	9492	9492
Families	50402	50402	4746	4746
R-squared	0.05	0.05	0.00	0.00

Notes: * p<0.1, ** p<0.05, *** p<0.01. We only include parents without national first given name. Clustered standard errors at the family level. Treated cities: Hanover and Frankfurt. Control cities: Heidelberg, Luebeck, Nuremberg, and Mannheim. Results based on equation 1.1.

We think that our empirical approach can be easily applied to many other contexts. Using name choices as indicators for identity change, and exploiting variation within families to control for unobservable characteristics rests on data, which is available in many contemporary and historical settings. Our findings based on the nation-building policies of the Prussian elites around 1815 suggest that such policies can shape identities and likely contributed to the emergence of a national identity in Germany.

Chapter 2

When autocrats fail: Bismarck's struggle against the socialists

In this paper, I analyze Bismarck's attempts to reduce support for the revolutionary socialist party by introducing, first, a redistributive social insurance scheme for workers and, second, repressive anti-socialist laws. Overall, I find that Bismarck failed in reducing the support for the socialist party. To the contrary, the socialist party gained in constituencies more affected by his policies. For identification, I exploit local and industry-specific variation in treatment intensity due to ex-ante existing local healthcare. This variation allows me to use a difference-in-differences as well as a shift-share approach. I explain the result by an unintended consequence of the social insurance: one particular form of health insurance (so-called auxiliary funds) allowed the socialist party a new way of political organization in an otherwise repressive political environment.

2.1 Introduction

How can autocratic elites curb revolutionary threats? Scholars at least since Machiavelli and Tocqueville dealt with this question and proposed different answers. Applying a "carrot and stick" strategy are among the most prominent solutions. China's policies combining ambitious health care reforms with a new form of repression through the social credit system are arguably one prime example of this strategy in the present. However, there is a lack of evidence about whether this strategy works in favor of the ruler.

In this paper, I analyze the effects of Bismarck's policies against the revolutionary threat of the socialist party. This is arguably one classical case of an autocratic regime deciding in favor of redistribution and repression. In the 1870s, vote shares for the socialist party hiked, support for the Paris commune increased among the German society, and employers saw unseen levels of strike activities. In response, Bismarck's government introduced the so-called *Sozialistengesetze* (anti-socialist laws) in 1878, which banned socialist organizations and led to countless imprisonments. To complement this repressive policy with an integrative one, five years later the government introduced the world's first public social security scheme specifically targeted at blue-collar workers. Bismarck explicitly stated his political motivation in parliament: "Give the worker the right to work as long as he is healthy, give him care when he is sick, give him care when he is old. When you do that [...], then I believe that the gentlemen of the Wydener program [the socialist party] will blow their whistle in vain." (Bismarck, [1884]1910, own translation). In contrast to Bismarck's goal, I find that the socialist party particularly gained in constituencies in which more voters were eligible for social insurance. Moreover, the socialist party did not lose votes in constituencies particularly affected by the anti-socialist laws.

I examine whether his policies of repression and redistribution lead to declining or increasing support for the socialist party during Bismarck's reign as chancellor until 1890. Using Prussian census data and new archival data, I examine two main outcomes: (i) votes in the general elections, and (ii) distribution of illegal newspapers. The main challenge here is to entangle the general role of blue-collar workers, which formed the core of voters for the socialist party, from the specific effect of the social insurance, for which only blue-collar workers were eligible.

To do so, I exploit the fact that local and industry-specific insurance schemes existed already before the introduction of Bismarck's public scheme. To account for this variation, I rely on a detailed and previously unused firm census of 1876. Around 35% of all blue-collar workers already had health insurance before Bismarck's reform. Employers often co-financed the ex-ante existing health insurances. These schemes are seen as a role model for Bismarck's policies and became redundant with the introduction of the public insurance scheme. Using a difference-in-differences framework, I compare changes in votes in constituencies where Bismarck's health insurance lead to large additional insurance coverage with regions with less of a coverage increase.

As a second identification strategy, I construct a shift-share instrument. For this, I rely on industry-specific variation in the share of ex-ante insured workers. The company census and other sources allow me to calculate the share of ex-ante insured workers in 11 industries. I combine these shares and the local employment shares to build my shift-share instrument.

The results of both identification strategies show that Bismarck's carrot did not work out the way he planned. On the contrary, the introduction of health insurance led to increasing votes for the socialist party and more demand for the illegal revolutionary newspaper *Der Sozialdemokrat* which I use as indicator for revolutionary activity. Crucially, these results are conditional on allowing for flexible effects for the share of blue-collar workers. The magnitude of the effects of social insurance are quite large: In a constituency with a higher share of newly-insured workers of one standard deviation, the socialist party increased their vote share by 3.75 percentage points in comparison to an average constituency in terms of newly-insured workers. This effect explains around 70% of the average increase in votes for the socialist party when comparing the elections before and after the introduction of the social insurance.

To capture the effect of anti-socialist laws introduced in 1878, I compare electoral constituencies with and without banned socialist organizations. Historical accounts highlight that the ban on their organizations hit the socialist party hardest. However, also the "stick", shows not the intended results on vote shares for the socialist party.

The central identification concern is that the local industry shares that form the basis of my instrument and treatment variable predict votes for the socialist parties through channels other than the health insurance. I follow an approach of Goldsmith-Pinkham *et al.* (2020) and construct pre-trend figures by industry to preclude other channels. I do not find evidence for systematic and significant pre-trends for the different industries. Furthermore, my findings suggest that indeed industries with a low share of ex-ante insured workers drive my results. To provide one example, I find a notable difference in votes for the socialist party between the otherwise similar industries of textiles and clothing after the introduction of social insurance. I do not find a correlation between blue-collar workers in textiles (with a high share of ex-ante insured workers) and votes for the socialist party after the introduction of Bismarck's policies. However, I do find a positive correlation between blue-collar workers in clothing (with a low share of ex-ante insured workers). This supports my claim that, indeed, the introduction of health insurance is the main channel for the rising support for the socialist party.

Seeking explanations, I focus on the implementation of the health insurance scheme and argue that the socialist party used one particular form of health insurance – so-called *Hilfskassen* (voluntary health funds) – as a new platform for political organization in times of a repressive political environment. I show that gains for the socialist party were highest where the voluntary health funds were strongest. Employees organized these voluntary health funds and chose to join even though employers did not have to contribute financially (in contrast to the majority of public insurance schemes). Thus, workers were even willing to pay a price to join voluntary health funds.

This paper makes three main contributions. First, I add to the empirical literature in political economy on how autocratic rulers deal with revolutionary threats. Following Acemoglu and Robinson (2000) , authoritarian rulers can choose between democratic reform and redistribution. Existing empirical research focuses on the former (e.g., Aidt and Franck, 2015; Brückner and Ciccone, 2011). I contribute to this literature an empirical investigation of a classical scenario in which the rulers opted for redistribution.[15] I show that this strategy can fail due to loopholes in the implementation and the inherent moral hazard in redistribution policies.

Second, there is a related broader literature in political economy on policies of governments to gain support, for instance, by providing of material benefits, assimilation policies or propaganda. Empirical analyses studying the effect of the provision of material benefits on support for the government are most related to my study. Recent contributions show that governments can successfully buy political support by land redistribution (Caprettini *et al.*, 2019) and increase war activity by welfare spending (Caprettini and Voth, 2020). I contribute to these studies by analyzing a setting in which the government tries to win over voters that it previously has targeted as "enemies of the empire" (Bismarck's description of socialists). My main conclusion is that Bismarck was not able to reduce support for the SPD. This study possibly shows the limits of redistribution policies to increase voter's support.

Third, I provide the first empirical evaluation of the political consequences of Bismarck's social policies. The existing literature in economic history focuses on its effect on mortality (Bauernschuster *et al.*, 2019) , savings (Lehmann-Hasemeyer and Streb, 2018), fertility (Scheubel, 2013; Guinnane and Streb, 2019) , and out-migration (Khoudour-Casteras, 2008). There is a consensus in this literature that the introduction of the social insurance led to social improvements, for instance, a decline in mortality (Bauernschuster et al. 2019), and formed the basis of the welfare state in Germany until the present.

The rest of the paper is organized as follows. Section 2.2 provides some historical background about the rise of the socialist party and Bismarck 's policies to reduce the support for the socialist party. In Section 2.3, I describe my data and explain how I measure the share of newly insured workers and how I capture the anti-socialist laws. Section 2.4 presents the empirical strategy as well as my main results. In Section 2.5, I explore possible mechanisms. Section 2.6 concludes.

2.2 Historical background: Bismarck and the social democrats

In the 1870s, after the German unification in 1871, the socialist movement and the *Sozialistische Arbeiterpartei Deutschlands* (SAP; Socialist Workers' Party of Germany) gained in popularity among the German society. In 1875, the party was founded as a coalition of the previous

[15] Acemoglu and Robinson (2000, p.1185) also discuss this example.

workers' parties in Germany, the *Sozialdemokratische Arbeiterpartei* (SDAP; Social Democratic Workers' Party) and the *Allgemeine Deutsche Arbeiterverein* (ADAV; General German Workers' Association). In the next general election in 1877, the party received 9.1% of the votes after only 3.2% six years earlier. Their core demands laid down in the *Gothaer Programm* were the introduction of a 10-hour-working day, the prohibition of Sunday-work, more rights for trade unions, and more education, especially for younger workers (Bartel *et al.*, 1980, p.30ff).

Support for the Paris Commune and unseen levels of strike activity (at least 530 strikes between 1871 and 1873) increased the bourgeoisie's fear of a socialist revolution. Chancellor Bismarck, for instance, fuelled the bourgeoisie's fears by spreading rumors that supporters of the SAP formed a kind of army in the middle of Germany that even limited the economic recovery after the Gründerkrise (Bismarck, [1878]2004). Bebel, the leader of the socialist party, triggered the fears by famously claiming in parliament "that before a few decades pass, the battle cry of the Parisian proletariat 'War to the palaces, peace to the huts, death to misery and idleness!' will become the battle cry of the entire European proletariat" (Bebel, [1911]2013, p.184, own translation).

Bismarck's government reacted harshly to the growing socialist movement by introducing the so-called *Sozialistengesetze* (anti-socialist laws) in 1878. The anti-socialist laws prohibited most activities of the SAP and trade unions. Bismarck aimed to conduct a "war of extermination" with laws against the "red enemies of the empire" (Bismarck, [1878]2004). Overall, the anti-socialist laws led to a ban on more than 200 associations and 400 publications within a year. The implementation was harsh and did not differentiate between social entertainment clubs, trade unions with a strong focus on local economic assistance on the one hand, and socialist agitators on the other hand (Lidtke, 1966, p.80). However, supporters of the banned party were still allowed to run for parliament as private persons. Centralized party activities like campaigning were forbidden.

These laws were a strong blow to the socialist party, and let to numerous discussions on how to react. To circumvent the prohibition by law, the party directly dissolved all official party structures. However, the party's leadership was in disagreement about how to further react to the anti-socialist laws and divided into three main camps (Engelberg, 1959). Reformists suggested following the rules to achieve an early repeal of the anti-socialist laws while anarchists distanced themselves from the party and rejected participation in elections. The Marxists, however, suggested a dual strategy of legal and illegal activity. In the early 1880s, the Marxist approach became the official guideline of the party also because the government decided to prolong the anti-socialist laws. August Bebel and Karl Liebknecht were the leading figures behind this strategy. This new strategy required a new organizational structure that – in contrast to the former rigid lead from above – was locally rooted much to the delight of Friedrich Engels (Bartel *et al.*, 1980, p.61ff). It was possible with this strategy to retain the same number of seats in parliament n the 1881 election.

To complement the repressive policy, Bismarck's government introduced a new social policy to reduce the support for the socialist party in 1883 . Bismarck formulated his goal bluntly: "Give the worker the right to work as long as he is healthy, give him care when he is sick, give him care when he is old. When you do that [...], then I believe that the gentlemen of the Wydener program [the SAP] will blow their whistle in vain." (Bismarck, [1884]1885a, own translation). Moreover, he argued that preventing a revolution by redistributive social policy would be cheaper than a redistribution of property in case of a revolution.[16] Bismarck also attempted to strengthen the link between the workers and the nation-state (Kocka, 2016). Thus, the introduction of a new social insurance was at the same time a policy against the socialist party and a nation-building policy.

The social insurance built upon three pillars: health insurance, accident insurance, and pension insurance. The *Gesetz betreffend die Krankenversicherung der Arbeiter* enacted June 15, 1883, and implemented December 1, 1884, provided public health insurance coverage for blue-collar workers in non-agricultural sectors. It covered, for instance, sick pay and doctor appointments with employers paying one-third of the contributions and employees two-thirds. The health insurance stood at the core of the social insurance reform. One year later, the government provided accident insurance, targeted at the same workers group. However, in terms of material benefits, it was smaller in scope than health insurance. Moreover, the criteria for obtaining this insurance were quite strict (Ritter and Tenfelde, 1992, p.698). The pension insurance was implemented 1889, shortly before Bismarck's reign as chancellor ended. Overall, these policies targeted the workers and not the working poor, and, thus, the core voters of the socialist party (Ritter and Tenfelde, 1992, p.702).

Before Bismarck's reform, insurance coverage existed but was dependent on local and industry-specific decisions. For instance, Knappschaften (miners' guilds) offered health insurance for miners, and Fabrikkassen covered health insurance for the employees of firms. Employers partially contributed to the financing of these insurance programs. These existing health insurance schemes will later be crucial part of my empirical strategy.

It was important for the socialist party to take a clear position on the new social security policies. On the one hand, better coverage corresponded to their demands, on the other hand there was a risk of losing votes. Indeed, contemporaries and potential voters interpreted the social reforms as 'socialist'. Therefore, the socialist party needed a sharp distinction from Bismarck's policies to not lose ground. Consequently, they rejected the public social insurance. The party put forward three main arguments in their illegal official newspaper *Der Sozialdemokrat*: Firstly, workers in agriculture did not get social insurance. Secondly, the laws implied substantial control by employers and the state. Thirdly, it did not lead to any improvement in political participation rights and occupational safety (Der Sozialdemokrat, [1883]2009).

[16] For similar reasons, the business elites were in favour of these policies because they did not include stricter industrial safety (Paster, 2013).

When it comes to the political evaluation of Bismarck's policies against the socialist party, the historiography remains inconclusive. Some historians (most notably Wehler, 2006, p.915) claim that Bismarck's introduction of social insurance – at least in the long-run – successfully integrated the working class in the new German nation-state. Others argue that the opposite was the case because of the small distributional effect (Ritter, 1983, p.50ff). There is a broad consensus, however, that the introduction of the anti-socialist laws was not successful for Bismarck. Wehler (2006, p.906) argues that the anti-socialist laws led to increasing solidarity among the SAP voters and not to deterrence effects. Nipperdey (1992, p.400) concludes that the anti-socialist laws were too excessive because they solely relied on violence and coercion and made supporters of the SAP enemies of the state. Interestingly, Lidtke (1966, p.71) argues that the lack of success of the social insurance was due to the sharp anti-socialist laws. However, the literature lacks a quantitative analysis of the effects of Bismarck's politics of *Zuckerbrot und Peitsche* (carrot and stick). The following section describes the data to shed light on the quantitative evaluation of Bismarck's policies.

2.3 Data

My empirical analysis builds on several new datasets. Data from authoritarian countries are often not trustworthy. In the case of Imperial Germany, however, the data quality is regarded quite high, and different datasets have often been used for empirical analysis in economic history.

Health Insurance

To capture the effect of Bismarck's social insurance, I focus on the public health insurance scheme. The main reason for this is that health insurance played the biggest role financially (Khoudour-Casteras, 2008, p.234ff). The other two pillars of social insurance were much more limited. The accident insurance had strict conditions under which workers received compensation. The pension insurance set a retirement age that significantly exceeded the average life expectancy, and its implementation started after Bismarck resigned as chancellor.

For my analysis, I collected new data from multiple sources to calculate health insurance coverage before Bismarck's reform and subsequently the share of newly insured workers via the public health insurance. Three groups must be taken into account: firstly, employees in (larger) companies, secondly, employees in the small workshops, and thirdly, employees in the mining industry who play a special role. In the following, I describe how I calculate the share of already insured employees for each of these three groups.

To capture employees in larger companies, my main source is an official firm census conducted by the Minister für Handel, Gewerbe und öffentliche Arbeiten (1876a). From this census, I obtain information on every firm with more than 30 workers that offered health insurance

to its workers before Bismarck's reform in Prussia. The census aimed to provide a detailed report on the social and economic situation of the workers and was conducted because of the growing concerns over the rising popularity of trade unions, the socialist party, and strikes. The data includes information on the number of workers employed and their sector. Information on the location of the firm helps me to link the data with electoral constituencies. In total, 2.067 firms offered health insurance for 361.574 employees in 1876. While this firm census provides a complete picture for bigger firms, also workers in smaller firms were already covered by health insurances (around 142.000 workers). Here, I rely on official data on the district-level provided by the (Minister für Handel, Gewerbe und öffentliche Arbeiten, 1876b). I distribute workers and firm from the district level to constituencies and sectors using the share of insured workers for each industry from the firm census and the share of workers for each industry in one constituencies within one district.

Moreover, some workshops already provided health insurance as well. To cover health insurance in workshops, I again rely on official data on the district-level provided by the Minister für Handel, Gewerbe und öffentliche Arbeiten (1876b). These insurances cover 157.000 workers. I distribute these already insured workers proportionally according to the proportion of workers in workshops in a constituency relative to all workers in workshops in a district.

Finally, I take into account that *Knappschaften* (miner guilds) provided mandatory health insurance for all workers in mining (Jopp, 2011). For instance, in 1878, 85 miner guilds provided health care for 261.118 workers in more than 3.000 mines, smelting works, and salt works. It is noteworthy that the miner guilds were heavily concentrated in the coal regions in the Ruhr area, Silesia, and Saxony. Following Guinnane and Streb (2019), I assume that all blue-collar workers in mining already had health insurance.

Based on these sources, I estimate that around 35% of all blue-collar workers were already insured. Note that this share varied significantly between different sectors (see Table 2.1). While about 60% of all blue-collar workers in textiles already benefitted from ex-ante existing health insurance, only 18% of all blue-collar workers employed in the construction and timber industry were covered by health insurance. I will use these differences across industries for my empirical strategy.

Firms had several reasons to support health insurance (Frevert, 1984, p.185-219). Importantly, there were economic arguments, e.g., that the provision of health insurance increases productivity. In addition, firms hoped to strengthen the loyalty and discipline of the workers in times of high levels of turnover. These *Fabrikkassen* and similar forms of health insurance had a long tradition in Germany, which went back to the beginnings of the industrialization. Thus, the amount of ex-ante existing health insurance does not reflect an immediate reaction of employers to the growing popularity of the socialist party in the 1870s.

Relying on these sources implies one assumption: The German parliament introduced the public health insurance in 1884 and the data for ex-ante existing health insurance is from 1876. Thus,

TABLE 2.1
EX-ANTE EXISTING HEALTH INSURANCE BY INDUSTRY

Ex-ante existing health insurance by sector	in %
Mining	100.00
Stones	24.41
Metal and machinery	53.15
Chemistry	55.40
Textiles	60.91
Paper and leather	41.35
Food	29.70
Clothes	18.54
Construction and timber	18.09
Print and art	30.06
Other	15.89

Notes: The table reports the share of ex-ante health insured workers by industry.
Sources: Own calculation based on Minister für Handel, Gewerbe und öffentliche Arbeiten (1876a) and Minister für Handel, Gewerbe und öffentliche Arbeiten (1876b).

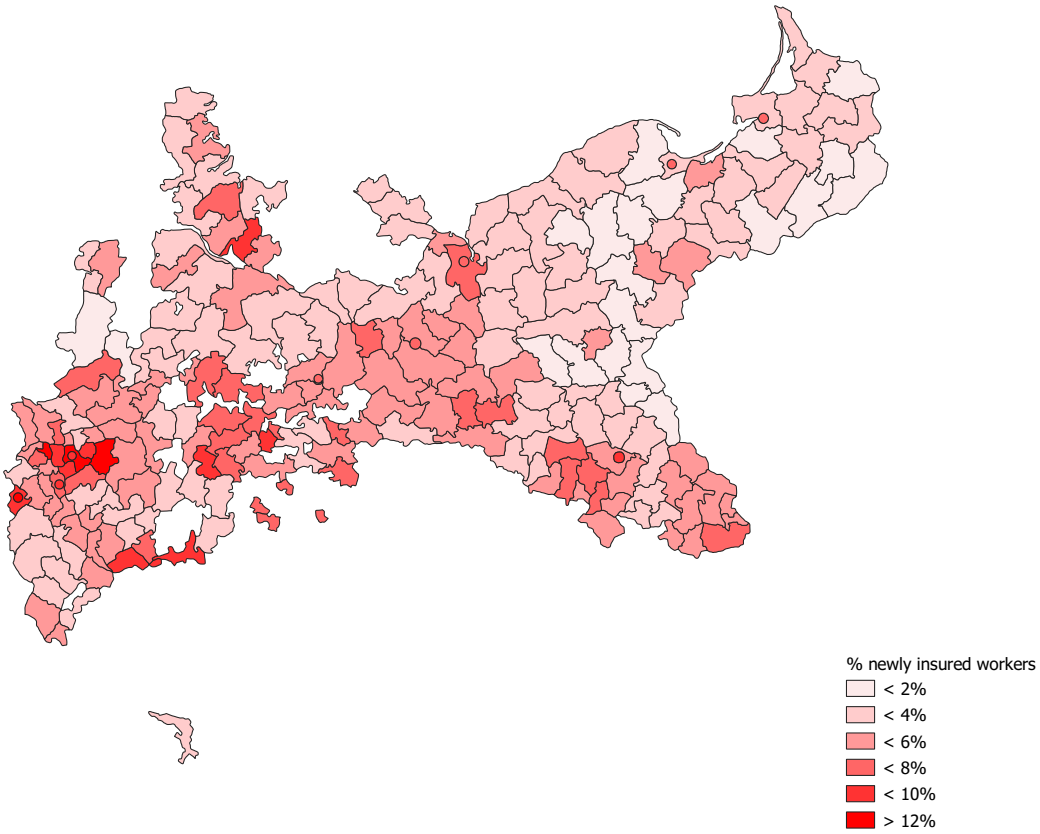
I must assume that the share of already insured workers remains stable between 1876 and 1883. Given the flat development of membership in ex-ante existing health share in the years before 1876 (Minister für Handel, Gewerbe und öffentliche Arbeiten, 1876b) (Bauernschuster *et al.*, 2019, p.10) and the lack of major institutional changes, this assumption seems plausible.

As a final step, I calculate the share of newly insured workers at the level of electoral constituencies. I combine data on ex-ante existing coverage of health insurance with information on blue-collar workers from Galloway (2007). The number of blue-collar workers captures the share of the workforce that was eligible for public health insurance. I subtract the number of workers with ex-ante existing health insurance from blue-collar workers in each constituency and weigh this number by population of each constituency. The share of newly insured workers was, on average, around 5% of the local population (see Table 2.3). Figure 2.1 shows the share of newly insured workers for each constituency. Broadly speaking, the share is higher in constituencies of industrialized regions.

Anti-socialist laws

To quantitatively operationalize the anti-socialist laws , I collect data on all forbidden organizations (in total 245) after the introduction of the anti-socialist law in 1878 until 1879. Historical accounts highlight that the ban on their organizations hit the socialist party hardest (Tennstedt, 1983, p.284). By focusing on forbidden organizations, I intent to capture those constituencies, in which the laws especially hit members of the socialist party.

FIGURE 2.1
NEWLY INSURED WORKERS



Notes: Map of share of newly insured workers (in the population) in Prussia. More red constituencies experienced a higher share of newly insured workers.
Sources: Own calculation based on Minister für Handel, Gewerbe und öffentliche Arbeiten (1876a), Minister für Handel, Gewerbe und öffentliche Arbeiten (1876b), and Galloway (2007).

My main source is information published in the official *Reichsanzeiger* and collected by Teich (1879). This list includes the type and location of the organization. All of these associations were prohibited on the basis of the socialist law, i.e. they had links with the socialist party or the trade union. There was a wide range of forbidden associations: explicitly political associations (27%), so-called workers' associations and electoral associations, but also social associations (33%), including singing associations, as well as other groups of associations (see Table 2.2). Based on this information, I construct a dummy variable for all constituencies with at least one forbidden organization. Overall, 29 of all 228 electoral constituencies in Prussia were affected.

Table 2.2
Types of Forbidden Organizations

Forbidden socialist organizations by type	in %
Political	27.05
Local Support	11.48
Social clubs (e.g. singing)	33.20
Occupation (e.g. carpenter)	11.89
Education	6.97
Other	9.02

Notes: The table reports the share for different types of forbidden organizations.
Sources: Own calculation based on Teich (1879).

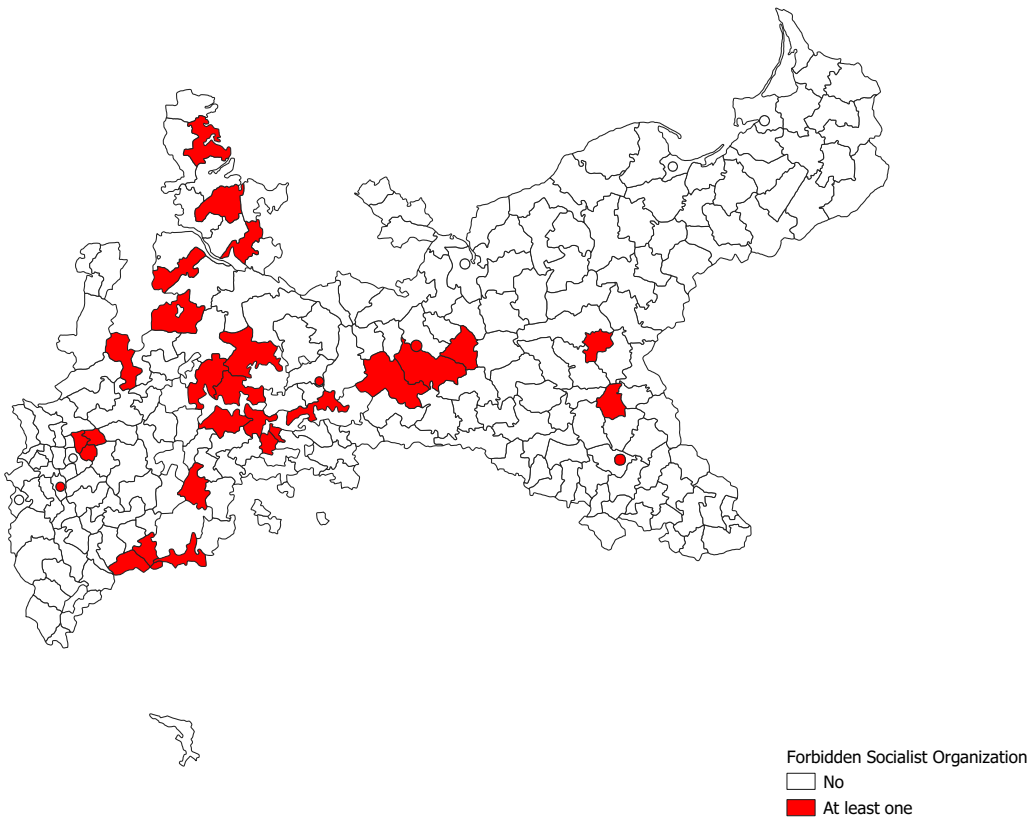
There are at least two caveats. Of course, this variable does not capture every aspect of the anti-socialist laws. For instance, the anti-socialist laws led to a ban of most socialist newspapers and magazines, which probably affected more constituencies. Unfortunately, data on the distribution of these newspapers do not exist. Moreover, I cannot rule out that members of banned organizations lived in neighboring constituencies. However, given that constituencies often consist of a city and the surrounding area, this potential issue should be less problematic.

Figure 2.2 shows that constituencies around Berlin, parts of Saxony, and northern Germany experienced bans of socialist organizations. Other comparable constituencies, which are also home to many industries, rarely were home of banned organizations, see, for instance, parts of Silesia and the Ruhr area.

Legal support for the socialist party: Votes in elections

The main dependent variable of all baseline regressions is vote shares for the socialist party in general elections made available by Caramani (2004). Given the prohibition of other legal activities, e.g., party membership, voting behavior is among the best available measurements for legal support for the SAP. Because the other parties often formed coalitions against the SAP in the runoff elections in the majority voting system, the SAP rarely won seats in the parliament

FIGURE 2.2
FORBIDDEN SOCIALIST ORGANIZATIONS



Notes: Map of forbidden socialist organization at the beginning of the anti-socialist laws. Red constituencies experienced at least one forbidden socialist organization.
Sources: Teich (1879).

despite quite high vote shares (Ritter and Niehuss, 1980, p.29). To account for this, I focus on the vote share and not whether the SAP won a seat in an electoral constituency.

Despite the overall monarchic character of the German Kaiserreich, the general elections were important for the government and the socialist party. The government needed a majority to enact laws. Bismarck, for instance, even dissolved the parliament to get a majority for the anti-socialist laws. For the socialist party, electoral successes allowed the only remaining public stage, namely debates in parliament.

Figure B1 and Table 2.3 show the overall development of the vote shares of the socialist party in the German Kaiserreich. After an initial rise until 1877, the vote shares declined after the introduction of the anti-socialist laws but recovered 1884 and 1887 and experienced a sharp increase in 1890, the last election before the anti-socialist laws were abolished. The SAP even got the most votes in this election.

Illegal support: The forbidden newspaper *Der Sozialdemokrat*

To capture other – more illegal and partially revolutionary – activities as well, I rely on the subscribers' list of the forbidden newspaper *Der Sozialdemokrat* (The Socialdemocrat). Press activity was important for political activities in the 1870s. This becomes also evident, given that six out of 26 paragraphs of the anti-socialist laws addressed the press and substantially limited the distribution of socialist newspapers. *Der Sozialdemokrat* was supposed to be the illegal alternative for the banned newspapers. It is, therefore, not surprising that interior minister Puttkamer described this newspaper as "one of the most dangerous attacks on the security and inner peace of the German Reich" (cited after Engelberg, 1959, p.51, own translation). The police tried to prevent its distribution and, for instance, incarcerated a socialist for six months after they were able to connect his handwriting to the delivery of 600 issues (Belli, 1956, p.125).

The newspaper fulfilled at least three main functions for the party in times of repression (Engelberg, 1959, p.65ff): First, the articles assessed the current political situation in Germany from a socialist perspective, for example, the anti-socialist laws and social insurance. Second, the content often dealt with the aforementioned ideological divides within the party. *Der Sozialdemokrat* was the central organ of the dominant Marxist wing. Thus, the distribution of the newspaper captures not only the local organization but also the radicalization of the party. Third, the newspaper was used for practical purposes, such as enabling communication between members, for instance, requests on missing (likely due to imprisonment) socialists.

The subscribers' lists are available in the digitalized archive of Julius Motteler, who was responsible for managing the complex and illegal distribution of Der Sozialdemokrat. Based on Motteler (2019), I calculate the number of subscribers for each constituency between 1887 and 1890. Unfortunately, this information is not available for other years. I plot the number of subscribers by constituency in Figure 2.3. While there are a lot of constituencies without sub-

scribers, constituencies around Berlin, in the Ruhr area, parts of Saxony and northern Prussia had quite numerous readership. When considering the number of issues sent to a particular city, one should keep in mind that it is reasonable to assume that not just the individual subscribers read the issues. Motteler himself estimated, on average, around ten readers of every newspaper issued, as the subscribers handed theirs to others (Stammberger, 1979, p.37).

TABLE 2.3
DESCRIPTIVE STATISTICS

	mean	sd	min	max
Votes for the socialist party in %				
Election 1871	2.55	7.31	0	47.06
Election 1874	4.64	9.94	0	58.64
Election 1877	5.54	10.30	0	51.03
Election 1878	3.42	7.77	0	46.29
Election 1881	2.71	6.81	0	39.08
Election 1884	4.71	9.69	0	51.66
Election 1887	5.64	10.11	0	53.44
Election 1890	12.23	14.58	0	66.11
Indicators for health insurance				
% Newly insured	4.97	2.15	1.30	14.59
% Newly insured instrument	4.74	2.16	1.21	11.34
% Blue collar	9.71	5.51	2.29	27.45
Indicator for anti-socialist laws				
Dummy forbidden socialist organization	0.13	0.33	0	1
Illegal activities				
Circulation of illegal newspaper *Der Sozialdemokrat*	235.46	1674.21	0	24568

Sources: See Chapter 2.3.

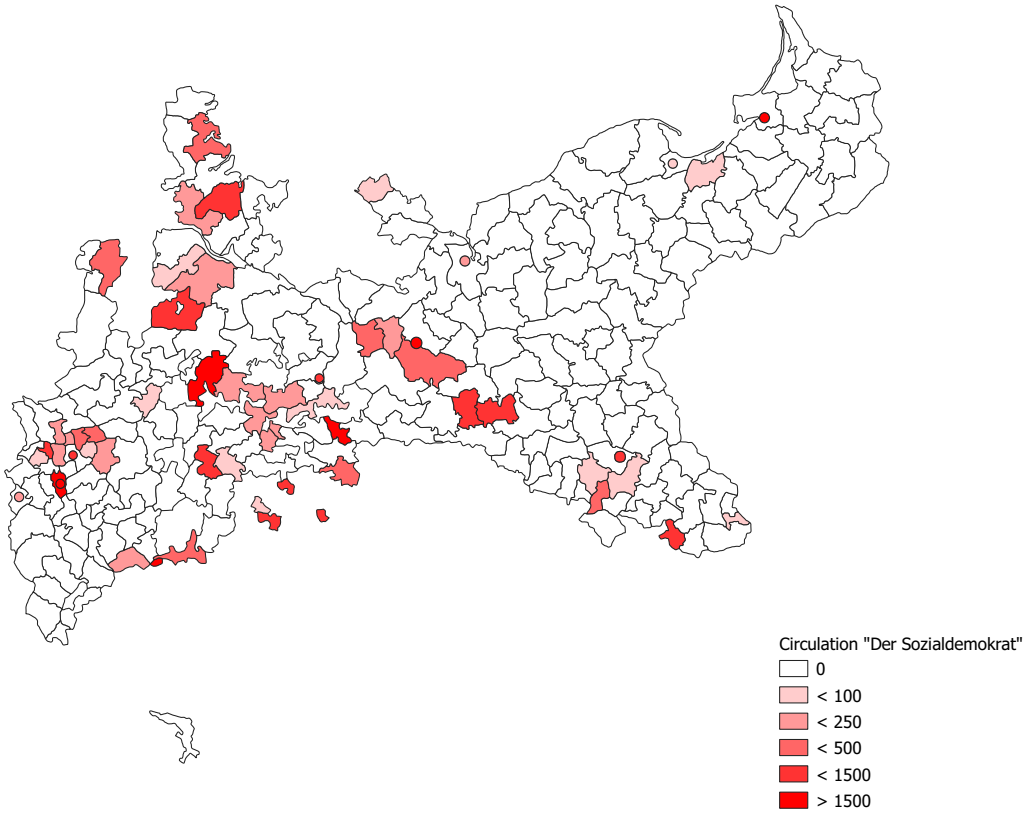
2.4 Empirical framework and results

In this section, I present evidence on the effect of the introduction of social insurance on changes in voting shares for the SAP. Here I rely, first, on a difference-in-differences setup and, second, on a shift-share instrument. The latter also allows me to discuss and deal with potential threats to identification. Next, I show the effect on the circulation of the forbidden socialist newspaper *Der Sozialdemokrat*. Subsequently, I investigate the effect of the anti-socialist laws before I also consider the interaction of the two policies.

The effect of social insurance: Difference-in-differences estimation

The empirical strategy is to compare changes in voting shares for the SAP in regions where Bismarck's health insurance lead to large additional insurance coverage with regions where there was less of an increase in a difference-in-differences setup (similar to Finkelstein, 2007). To do

FIGURE 2.3
DISTRIBUTION OF NEWSPAPER *DER SOZIALDEMOKRAT*



Notes: Map of the distribution of the illegal newspaper *Der Sozialdemokrat* between 1887 and 1890. Red constituencies experienced at higer distribution of the newspaper.
Sources: Motteler (2019).

so, I exploit spatial variation of health insurance rates before Bismarck's reform as described in Section 2.3. I estimate the following flexible difference-in-differences specification

$$SAP_{it} = \alpha_i + \theta_t + \sum_{t=1871}^{t=1890} \gamma_t(Treatment_i \cdot Year_t) + \sum_{t=1871}^{t=1890} \delta_t(BlueCollar_i \cdot Year_t) + X_{it}\beta + \epsilon_{it} \quad (2.1)$$

where the dependent variable SAP_{it} is the vote share for the SAP in electoral constituency i and year t. α_i capture electoral constituency fixed effects, θ_t election year fixed effects, and X_{it} various time-varying control variables like population provided by Galloway (2007). The key variables of interest are the interactions of the year fixed effects with the impact of Bismarck's health insurance, $Treatment_i \cdot Year_t$. The coefficients of this interaction (γ_t) show the difference in vote shares over eight elections in counties where Bismarck's insurance affected a larger share of the population relative to counties where the impact was smaller. Success for Bismarck's politics means a declining share of votes in constituencies with a higher share of workers affected by his policies after the health insurance is in place. It is important to highlight

here that voters could not anticipate the introduction of the health insurance in the election in 1881 because the famous *Kaiserliche Botschaft* announced the reform after the election. In my prefered specification, I also allow for a flexible effect of the share of blue-collar workers.

A key criterion for using geographic variation in existing insurance coverage for the workers to identify the political impact of Bismarck's reform is that this insurance was redundant of what the public health insurance subsequently covered. In line with this, Hänlein *et al.* (2009) argue that Bismarck used these existing insurance forms (for instance, so-called *Fabrikkassen*) as a blueprint and only added so-called *Ortskrankenkassen* (local health insurance).

Of course, health insurance rates before Bismarck's reform are not randomly assigned. Data from the 1882 census indicate that differences in industry employment can explain a substantial share of the cross-sectional variance in insurance coverage. The empirical approach is, therefore, to look at whether there is a break in any pre-existing differences in the trend of election outcomes around the introduction of Bismarck's public health insurance in 1884. The identifying assumption is that absent Bismarck's reform any pre-treatment differences would have continued on the same trends.

The results in Table 2.4 and its visualization in Figure 2.4 show that the socialist party mainly gained in constituencies with a higher share of newly treated workers after the introduction of health insurance. The effect is sizeable: an increase in additionally insured workers by one percentage point leads to a rise in the vote share of the SAP by at least 0.8 percentage point in 1884 (see columns 1 to 3). This effect increases in the next elections. By including population (in logs) in column 2, I can control for composition effects due to increasing migration and population growth in the urban centers. Column 3 shows my prefered specification. My measurement of newly insured workers is correlated with blue-collar workers. To address this potential caveat, I additionally allow the effect of blue-collar workers to vary over time in my preferred specification in column 3 of Table 2.4. The effect pattern for my treatment variable only changes quantitatively, but not qualitatively. The effect for the last election during Bismarck's reign of power decreases. To give some evidence for the magnitude of the impact, I calculate the share of the overall change between the election in 1881 and 1884 that can be explained by the effect of newly insured workers. On average, the socialist party gained 2 percentage points between 1881 and 1884. Multiplying the standard deviation of newly insured workers (2.15, see Table 2.3) and the effect size (0.82 for 1884, see column 3) yields that the socialist party increases their vote share by 1.76 percentage points in a hypothetical electoral constituency with a higher share of newly insured workers of one standard deviation compared to the election in 1881.

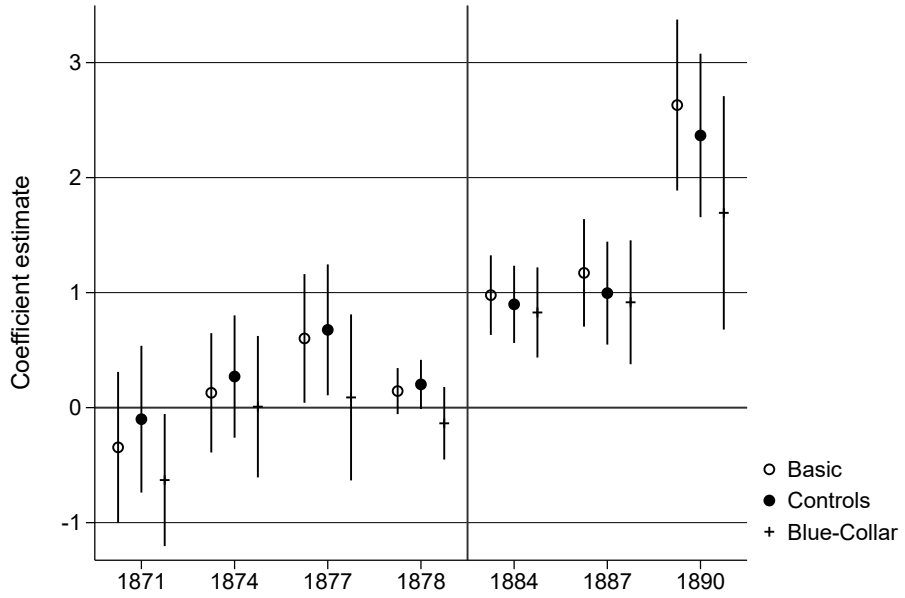
Note that there is no evidence for a significant pre-trend as the coefficients for the elections before 1884 remain mostly insignificant (except 1877 in column 1 and 2). Moreover, the point estimates do not reveal any clear trend, i.e., the pre-trend is not due to large standard errors. Note that the pre-trend looks even more stable in my prefered specification in column 3, which is reassuring for my identifying assumption.

TABLE 2.4
EFFECT OF SOCIAL INSURANCE, DIFF-IN-DIFF

Dep. var.: Votes SAP	Baseline (1)	Controls (2)	Blue-Collar (3)
% Newly Insured × 1871	-0.345	-0.100	-0.630**
	(0.323)	(0.314)	(0.283)
% Newly Insured × 1874	0.129	0.270	0.008
	(0.256)	(0.262)	(0.303)
% Newly Insured × 1877	0.602**	0.676**	0.089
	(0.275)	(0.280)	(0.356)
% Newly Insured × 1878	0.144	0.202*	-0.136
	(0.099)	(0.105)	(0.155)
% Newly Insured × 1884	0.978***	0.898***	0.827***
	(0.170)	(0.165)	(0.193)
% Newly Insured × 1887	1.171***	0.996***	0.916***
	(0.230)	(0.221)	(0.265)
% Newly Insured × 1890	2.631***	2.366***	1.694***
	(0.366)	(0.350)	(0.500)
% Blue Collar × 1871			0.283*
			(0.154)
% Blue Collar × 1874			0.140
			(0.115)
% Blue Collar × 1877			0.309**
			(0.126)
% Blue Collar × 1878			0.178**
			(0.081)
% Blue Collar × 1884			0.035
			(0.068)
% Blue Collar × 1887			0.038
			(0.090)
% Blue Collar × 1890			0.347
			(0.226)
Further Controls		✓	✓
County FE	✓	✓	✓
Time FE	✓	✓	✓
Observations	1824	1824	1824
Counties	228	228	228
Elections	8	8	8
R-squared	0.40	0.42	0.43

Notes: The omitted election is 1881. The dependent variable measures the share of votes for the social democratic party. Treatment variables: share of newly insured population in 1884, interacted with time dummies. Controls: Population (log). Standard errors, clustered at the district level, in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
Sources: See Chapter 2.3.

FIGURE 2.4
EFFECT OF SOCIAL INSURANCE ON VOTES FOR THE SOCIALIST PARTY



Notes: The figure plots the results from Table 1. The coefficients "basic" correspond to column 1 in Table 2, "controls" to column 2, "blue-collar" to column 3.
Sources: See Section 3.

The effect of social insurance: Shift-share analysis

To be more precise about the source of variation I exploit, I first describe the components of the variation in my main independent variable ($Treatment_i$) in the following equation:

$$Treatment_i = BlueCollar_i - ExAnteInsured_i = \sum_j BlueCollar_{ij} - ExAnteInsured_{ij} \quad (2.2)$$

The number of newly insured workers in county i consists of the sum (over all sectors j) of the difference between blue-collar workers and ex-ante insured workers. This sum, in turn, can be instrumented with a shift-share instrument of the following form

$$\sum_j \alpha_j (BlueCollar_{Prussia,j} - Insured_{Prussia,j}) \quad (2.3)$$

where α_j denotes the share of blue-collar workers in sector j and county i relative to total blue-collar employment in sector j.

Recent contributions on shift-share analysis (for instance, Goldsmith-Pinkham *et al.*, 2020) suggest differentiating between research designs relying on shocks and research designs relying

on shares. In my setting, the shares in employment for different industries lead to different exposure to the common shock of public health insurance. The idea is to ask whether differential exposure to public health insurance leads to differential changes in the outcome. Note that this empirical strategy does not assume the shares themselves to be uncorrelated with the political outcomes (Goldsmith-Pinkham *et al.*, 2020). I implement this shift-share instrument in the flexible diff-in-diff regression used in equation 2.1 and Table 2.4.

The results in Table 2.5 overall confirm the results from Table 2.4. The share of newly insured workers shows a strongly positive effect on votes for the socialist party after the introduction of the public health insurance in the second stage (column 1). The coefficients are larger compared to the OLS results in column 3. Note that I include flexible effects for the share of blue-collar workers throughout. Moreover, there is only weak evidence for significant pre-trends. All coefficients are insignificant (except for 1877, but only significant at the 10% level).

Threats to identification

The central identification concern is that the industry shares that form the basis of my instrument predict votes for the socialist parties through channels other than the health insurance. To address these worries, I follow the analysis by sector as proposed by Goldsmith-Pinkham *et al.* (2020). The main idea is to check whether industries with a low share of ex-ante insured workers indeed drive my overall positive effect.

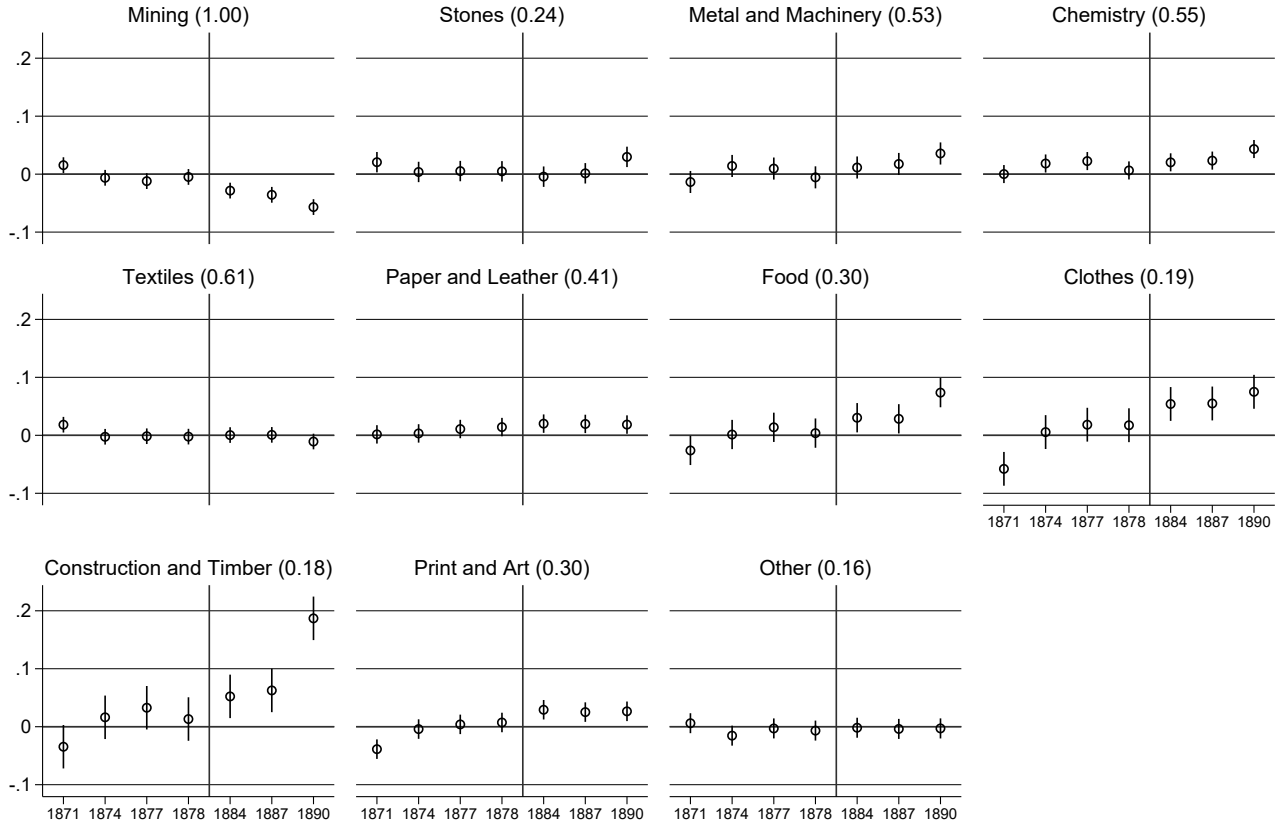
Therefore, I analyze the pre-trends for all sectors separately. In Figure 2.5, I keep blue-collar shares for 11 industries constant at the 1882 values and report the effect of these blue-collar shares on votes for the socialist party . In these regressions, I control for population (in logs), the overall share of blue-collar workers interacted with elections dummies, as well as time and constituency fixed effects. To allow comparability, I standardize the coefficients. In most cases, the pre-trends look satisfying with one exception: for some industries (clothes, food, and print/art), I observe a negative coefficient for 1871. However, this negative and significant coefficient is only visible for one year and the coefficients for the remaining years remain stable and insignificant. Although, of course, problematic, it should be mentioned that this was the first election in the newly formed German Empire and the socialist movement at that time consisted of two small parties which were only partially comparable to the socialist party later.

TABLE 2.5
EFFECT OF SOCIAL INSURANCE, SHIFT-SHARE IV

Dep. var.: Votes SAP	Second Stage (1)	Reduced Form (2)	OLS (3)
% Newly Insured × 1871	-0.557		-0.630**
	(0.375)		(0.283)
% Newly Insured × 1874	0.155		0.008
	(0.375)		(0.303)
% Newly Insured × 1877	0.708*		0.089
	(0.375)		(0.356)
% Newly Insured × 1878	0.265		-0.136
	(0.375)		(0.155)
% Newly Insured × 1884	1.429***		0.827***
	(0.375)		(0.193)
% Newly Insured × 1887	1.715***		0.916***
	(0.375)		(0.265)
% Newly Insured × 1890	3.397***		1.694***
	(0.375)		(0.500)
% Newly Insured Instrument × 1871		-0.490*	
		(0.277)	
% Newly Insured Instrument × 1874		0.139	
		(0.381)	
% Newly Insured Instrument × 1877		0.629	
		(0.374)	
% Newly Insured Instrument × 1878		0.236	
		(0.159)	
% Newly Insured Instrument × 1884		1.266***	
		(0.157)	
% Newly Insured Instrument × 1887		1.518***	
		(0.212)	
% Newly Insured Instrument × 1890		3.008***	
		(0.481)	
Further Controls	✓	✓	✓
County FE	✓	✓	✓
Time FE	✓	✓	✓
Observations	1824	1824	1824
Counties	228	228	228
Elections	8	8	8
R-squared		0.46	0.43

Notes: The omitted election is 1881. The dependent variable measures the share of votes for the social democratic party. Treatment variables: share of newly insured population in 1884, interacted with time dummies. Controls: Population (log) and blue-collar workers (share) interacted with time dummies. Standard errors, clustered at the district level, in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
Sources: See Chapter 2.3.

Figure 2.5
Pre-trend by Sector



Notes: These plots report pre-trends for all industries. The figures fix blue-collar shares as the 1882 values for 11 industries and report the effect of these blue-collar shares on votes for the socialist party. I control for population (in logs) and the overall share of blue-collar workers as in Table 2.4. As in the main specification, I include time and constituency fixed effects. The coefficients are standardized.
Sources: See Section 2.3.

For the effects after the introduction of public social insurance, evidence in favor of my identification strategy would show positive and significant effects for sectors with a lower share of ex-ante insured workers. The results in Figure 2.5 overall support this reasoning. I document the share of ex-ante insured workers in parentheses next to the sector. Clothes and construction/timber – two sectors with a low share of ex-ante insured workers – show the strongest standardized effects. The positive effects for blue-collar workers in print/art and food are smaller in magnitude, but still supportive of my overall case. Note that the effects of textiles with a high share of ex-ante insured workers (around 60%) remain almost constant. This finding is particularly interesting in comparison to clothes as both have a similar structure of employees, but differ in their share of ex-ante insured workers. The even adverse effects of mining also support my argument. These pattern illustrate that mainly constituencies with an ex-ante low share of insured workers drive my result. Overall, the pre- and post-trends provide sound support that the introduction of health insurance is the primary channel through which the socialist party gained votes.

The effect of social insurance on the newspaper *Der Sozialdemokrat*

To capture illegal political activity in times of the anti-socialist laws, I rely on the distribution of the newspaper *Der Sozialdemokrat* as my second main dependent variable. Unfortunately, the data do not allow me to run similar panel regression models, like in Table 2.4. Instead, I rely on cross-sectional analysis and aim to explore whether constituencies with a higher share of newly insured workers also have more subscribers of the newspaper.

The results in Table 2.6 show a strong positive effect of the share of newly insured workers on the existence of at least one subscriber of *Der Sozialdemokrat* (column 1) and also a strong effect on the number of subscribers in column 2 and 3. The size of the effect is quite large: a constituency with one percentage point more newly insured workers has on average more than 60 additional subscribers (on average, there were 235 subscribers in one constituency). I find even larger effects when I only look at constituencies with at least one subscriber in column 3. Note that all of these effects are conditional on the share of blue-collar workers. Overall, I interpret this evidence that a higher share of newly insured workers was also associated with more subscribers of *Der Sozialdemokrat*, the main outlet of the radical Marxist wing.

The effect of the anti-socialist laws

In addition to social insurance, Bismarck's policy also included the use of repressive anti-socialist laws. To account for this, I follow the approach of equation 2.1 and use as main independent variable a dummy variable, whether there was at least one forbidden organization. Again, I am interested in the changes of the dummy variable interacted with election years and rely on the following econometric specification

TABLE 2.6
EFFECT OF SOCIAL INSURANCE ON *DER SOZIALDEMOKRAT*

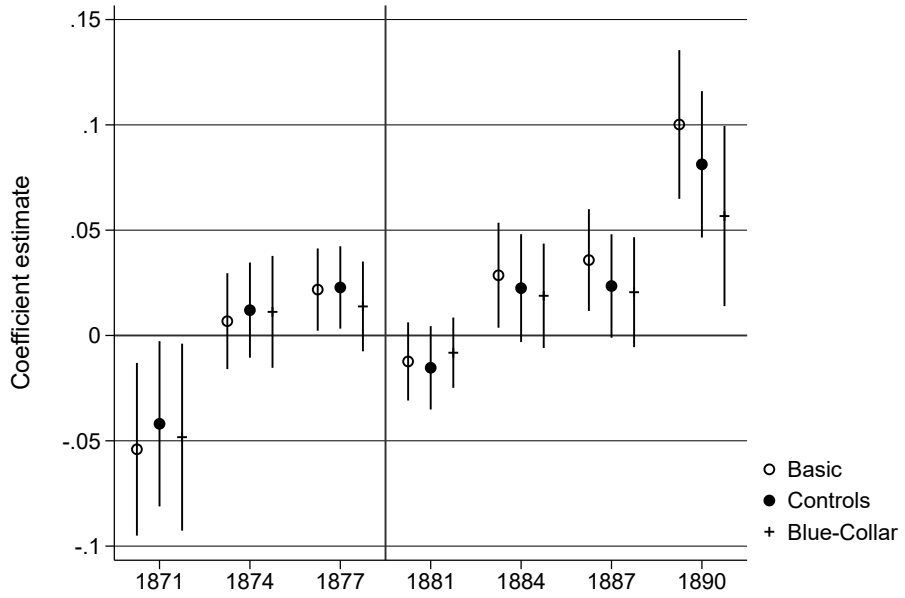
	Dummy (1)	Total (2)	Total (3)
% Newly insured	6.694***	62.311***	120.895**
	(2.216)	(18.123)	(48.594)
% Blue collar	1.636	1.352	-11.756
	(1.071)	(7.486)	(29.799)
% German speaker	-0.055	-0.314	-6.431
	(0.110)	(1.014)	(4.805)
% Protestants	0.216***	0.890	2.231
	(0.046)	(0.753)	(8.991)
Population (log)	0.229**	3.677**	6.932*
	(0.088)	(1.655)	(3.959)
Observations	228	227	51
R-squared	0.43	0.25	0.18

Notes: Dependent variable Column 1: Dummy for at least one subscriber of socialist newspaper; column 2: Number of subscribers (w/o Berlin); column 3: Number of subscribers, but only constituencies with at least one subscriber. Treatment variables: share of newly insured population in 1884. Standard errors, clustered at the district level, in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
Sources: See Chapter 3.

$$SAP_{it} = \alpha_i + \theta_t + \sum_{t=1871}^{t=1890} \gamma_t(AntiSocialisti \cdot Year_t) + \sum_{t=1871}^{t=1890} \delta_t(BlueCollar_i \cdot Year_t) + X_{it}\beta + \epsilon_{it} \tag{2.4}$$

In Figure 2.6 and Table B1, I present the results on the impact of the anti-socialist laws, which are overall mixed. Before the introduction of the anti-socialist laws in 1878, the socialist party particularly gained votes in constituencies in which the anti-socialist laws later prohibited socialist organizations. This result is plausible as one would expect more votes in constituencies with more socialist organizations. The difference becomes insignificant once I control for the share of blue-collar workers interacted with election dummies. With the anti-socialist laws in place, this trend stops and there is no statistical difference between constituencies with at least one forbidden organization and other constituencies in 1881. This pattern again changes in 1884 and the following elections (with no significant effects controlling for blue-collar workers in column 3). Interestingly, this change coincides with the introduction of social insurance. The difference between "treated" and "untreated" constituencies become similar to the difference observed before the introduction of the anti-socialist laws in 1887 and exceed the difference in 1890. Overall, I find that the anti-socialist laws led not to a decline in votes for the socialist party despite the harsh measurements. This finding is in line with the historiographic literature. In the next step, I explore the interaction of anti-socialist laws and social insurance.

FIGURE 2.6
EFFECT OF ANTI-SOCIALIST LAWS ON VOTES FOR THE SOCIALIST PARTY



Notes: The figure plots the results from Table B1. The coefficients "basic" correspond to column 1 in Table B1, "controls" to column 2, "blue-collar" to column 3.
Sources: See Section 2.3.

The interaction of repression and redistribution – the carrot and the stick

One of Bismarck's motives for introducing social security was to complement the harsh measures of the anti-socialist laws with social insurance to appease workers. It is therefore particularly interesting to analyze whether there are interaction effects between the two policies. To capture the interaction of the carrot (social insurance) and stick (anti-socialist law), the following equation captures not only the effect of social insurance and the anti-socialist law but also the interaction of the two.

$$SAP_{it} = \alpha_i + \theta_t + \gamma \cdot SocialInsurance_i \cdot Post1884_t + \delta \cdot AntiSocialist_i \cdot Post1878_t + X_{it}\beta + \epsilon_{it} \quad (2.5)$$

The results in Table 2.7 capture the effects separately, as well as the interaction of the two policies. Variation results again from differing treatment across constituencies. Column 1 shows that the SAP overall gained in constituencies where at least one organization was banned. In line with the results in Table 2.4, I find that the introduction of the carrot strengthened support for the socialist party (column 2). The results remain similar if I include both policies in column 3. To provide some magnitude of the effect of the social insurance in column 3, the average vote share in Prussian electoral constituencies increased from 3.7% before 1884 to 7.5% between 1884 and 1890. An electoral constituency with a higher share of newly insured workers of one standard deviation (2.1 percentage points) has a higher vote share for the SAP after the introduction of

the social insurance and in comparison to a hypothetical constituency with an average share of newly insured workers (4.9 percentage points) of 2.7 percentage points. Therefore, this effect accounts for roughly 70 percent of the overall observed increase in vote shares for the SAP.

Table 2.7
Interaction of Carrot and Stick

Dep. var.: Votes SAP	Stick (1)	Carrot (2)	Both (3)	Interaction (4)
Forbidden Organization × Post1878	0.022***		0.018***	-0.005
	(0.007)		(0.007)	(0.009)
Post1878	0.046***		-0.014***	-0.011**
	(0.006)		(0.005)	(0.005)
% Newly Insured × Post1883		1.294***	1.263***	1.136***
		(0.154)	(0.154)	(0.158)
Post1883		0.018***	0.033***	0.036***
		(0.007)	(0.007)	(0.007)
% Newly Insured × Post1883				0.506***
× Forbidden Organization				(0.146)
Further Controls	✓	✓	✓	✓
County FE	✓	✓	✓	✓
Time FE	✓	✓	✓	✓
Observations	1824	1824	1824	1824
Counties	228	228	228	228
Elections	8	8	8	8
R-squared	0.37	0.39	0.39	0.40

Notes: The dependent variable measures the share of votes for the social democratic party. Treatment variables: "carrot" share of newly insured population in 1884; "stick" dummy variable for at least one forbidden organization in the wake of the anti-socialist law. Controls: Population (log). Standard errors, clustered at the district level, in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
Sources: See Chapter 2.3.

Interestingly, the interaction of carrot and stick is strongly positive and highly significant in column 4. Here, the effect of the stick itself becomes insignificant. Thus, the socialist party gained in constituencies with at least one forbidden socialist organization and a higher share of additionally treated workers. I come back to this interaction effect in the next section.

To account for possible serial autocorrelation and the possibility of downward biased standard errors, I provide additional results where I collapse the data into pre- and post-period following Bertrand *et al.* (2004) in Table B2. The results are qualitatively and quantitatively almost identical.

Overall, the empirical results suggest that Bismarck was not able to reduce the votes for the socialist party. On the contrary, the results even provide evidence that the socialist party became particularly successful in those constituencies, where Bismarck's policies affected more workers and voters. Put differently, Bismarck completely failed with his political goal to reduce support for the socialist party. I put forward different explanations for this surprising and new finding in the next section.

2.5 Seeking explanations

When it comes to explaining the positive effect of social insurance on votes for the socialist party, I first highlight the role of voluntary health funds, which were part of the implementation of public health insurance and provided a new possibility of political organization in times of otherwise repressive politics. Second, I discuss alternative explanations.

Voluntary health funds as a new political organization

To explain the positive effect of the social insurance on votes for the socialist party, knowledge about the exact implementation of public health insurance is crucial. Insurance was organized in different forms at a local level, most notably local funds (*Allgemeine Ortskrankenkasse*), factory health insurance (*Fabrikkassen*), parish funds (*Gemeindekrankenversicherungen*), and voluntary health funds (*Hilfskassen*). All these organisations had to provide the same service. They were self-governed by a board consisting of representatives of the contributors. The voluntary health funds were especially attractive for politically active workers because (in contrast to other forms of health insurance) it allowed the workers to organize themselves independently of the company owners. Only employees were part of their governing board. In contrast, other forms of health insurance allowed employers to get access to the administration of health insurance. Thereby, they were able to better control workers linked with the socialists or trade unions (Tauchnitz, 1999). Exactly the aspect of autonomous workers' governance made this health insurance a popular instrument to circumvent the strict legislation of the anti-socialist law (Hänlein *et al.*, 2009, p.XXXV). A drawback was that employers did not contribute to this insurance form. Put differently, employees paid more to become member of the voluntary health fund. This cost makes it not only more astonishing that this form of insurance became popular, but also shows the importance workers attributed to their political freedom that the voluntary health funds provided.

Activists close to the socialist party organized more than 1000 events in Germany to convince workers to join the voluntary health funds (Hänlein *et al.*, 2009, p.XXXIV). August Bebel ([1883]2009, reprint), the leading politician of the socialist party, stressed the independence from employers as a key advantage to join the voluntary health funds in a speech in Cologne in front of 1000 workers in 1883. This campaign was a big success, as roughly 800.000 workers joined this type of health insurance in 1885 alone, accounting for 20 percent of all insured workers. The voluntary health funds were not only a form of health insurance but also a new form of political organization for workers, which was later replaced by the trade unions (Tennstedt, 1983, p.324ff). Unsurprisingly, the leading staff of the voluntary health funds were usually members of the socialist party.

Interestingly, the administration around Bismarck was well aware of this loophole. The Prussian minister of the interior Robert Viktor von Puttkamer ([1883]2009, reprint) who was in charge

of the central supervision of the anti-socialist laws, wrote to the local governments that they should prohibit speeches like the one in Cologne by Bebel and demanded to receive notifications in case of similar events. However, the government refused to change the law although Bismarck suggested to do so. Instead, they opted for stricter controls of the voluntary health funds. This decision was potentially influenced by Theodor Lohmann, the leading advisor of Bismarck for the social insurance reform (Hänlein *et al.*, 2009, p.XXXVIII). Lohmann was in favor of keeping the more liberal aspects of the reform. In addition, he doubted that there would have been a majority in parliament to ban the voluntary health funds (Tennstedt, 1983, p.334).

Overall, the voluntary health funds enabled a new form of political organization for the socialist party in the repressive environment of the anti-socialist laws. Following this argument, I hypothesize that the socialist party gained more in constituencies in which the voluntary health funds were stronger in membership.

To provide some quantitative evidence for this hypothesis, I analyze whether the socialist party particularly gained in regions in which a higher share of people joined the voluntary health funds. Here, I rely on the official health insurance statistics, which differentiate between the different forms of health insurance (Kaiserliches Statistisches Amt, 1887). These statistics are only available at the district level (a geographical unit higher than constituencies).[17] To identify the effect of membership in those voluntary funds on SAP vote shares, I estimate the following equation

$$\begin{aligned} SAP_{it} = \alpha_i + \theta_t + \gamma \cdot VoluntaryHealthFunds_i \cdot Post1884_t \\ + \theta \cdot OtherInsurances_i \cdot Post1884_t + X_{it}\beta + \epsilon_{it} \end{aligned} \tag{2.6}$$

The results in Table 2.8 support my argument: the socialist party mainly gained in districts with a higher share of voluntary health funds. Note that I control for the share of newly insured workers (excluding members in voluntary health funds) in column 1 and for the share of blue-collar workers (excluding members in voluntary health funds) in column 2. Both coefficients are insignificant, while the coefficient for the share of members in voluntary health funds is positive and significant.

The voluntary health funds became particularly popular in regions with more forbidden organizations as shown in Figure B2. I interpret this correlation as evidence that former active members of banned socialist groups used voluntary health funds as a new form of political engagement. The result helps to explain the positive interaction effect between forbidden socialist organizations and the share of newly treated workers in Table 2.7. The finding is in line with historical research that also highlights the continuity between membership in forbidden socialist

[17] Unfortunately, similar statistics on the county-level are not available (based on archival research in the Geheime Preussische Staatsarchiv).

TABLE 2.8
VOLUNTARY HEALTH FUNDS AND VOTES FOR THE SOCIALIST PARTY

Dep. var.: Votes SAP	Newly Insured (1)	Blue Collar (2)
Voluntary Health Funds × Post1883	0.858**	1.112***
	(0.385)	(0.377)
Newly Insured (excluding voluntary) × Post1883	-0.188	
	(0.183)	
Blue Collar (excluding voluntary) × Post1883		0.099
		(0.139)
Post1883	0.071***	0.068***
	(0.010)	(0.011)
Further Controls	✓	✓
District FE	✓	✓
Time FE	✓	✓
Observations	288	288
Counties	36	36
Elections	8	8
R-squared	0.66	0.66

Notes: The dependent variable measures the share of votes for the social democratic party. Controls: population (log). Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
Sources: See Chapter 2.3.

organizations and strength of voluntary health funds (Tennstedt, 1983, p.325). One can interpret this result as persistence in protest which has also been stressed in recent contributions on protest activity, for instance, by Bursztyn *et al.* (2020).

If the organization in voluntary health funds was an important channel for the electoral success of the SAP, I would expect that the effect of the newly insured workers becomes smaller and eventually insignificant when the anti-socialist laws were no longer in place after 1890. Therefore, I include the elections after 1890 in the setup of Table 2.4. The results in Table B3 support my argument as the coefficients become smaller, especially after 1900, and less significant. However, there seems to also be some persistence in the effect of newly insured workers.

Next, I would expect that the share of workers insured in the voluntary health funds to decline when the anti-socialist laws stopped since workers in these funds had to pay more than in other health funds. Indeed, after the initial success, this insurance became less popular. The share of all workers insured in voluntary health funds declined from 20% to 15% between 1885 and 1890 (based on own calculation from Kaiserliches Statistisches Amt, 1887 and Kaiserliches Statistisches Amt, 1892) und further dropped afterwards when the anti-socialist laws were repealed.

Summarizing, the evidence put forward shows that the introduction of the health insurance allowed – as an unintended consequence – the socialist party a form of political organization in

times of otherwise repressive politics. Other explanations are discussed in the remaining part of this section.

Further explanations

The theoretical literature in political economics has put forward the hypothesis that there might be a moral hazard if rulers choose to redistribute (Svolik, 2012). The leaders of the socialist party used this strategy. Facing a dilemma in parliament to support a government that implemented the anti-socialist laws or to refuse a policy that aimed to improve the living conditions of the workers, they chose to vote against Bismarck's social insurance. Instead, they demanded further legislation (most notably a working conditions act) and argued that Bismarck's policies were missing crucial aspects of their ideas. At the same time, they were able to sell Bismarck's social policies as their success, as Bismarck acknowledged in parliament that "if there were no social democracy, and if a lot of people were not afraid of it, the modest progress we have made so far in social reform would not exist" (Bismarck, [1884]1885b). The socialist party often mentioned this quote from Bismarck in their election campaigns. This explanation also helps to understand the lack of success of the carrot.

There is at least one alternative explanation for the lack of success of Bismarck's carrot. Khoudour-Casteras (2008) presents some evidence that the introduction of social insurance led to a decline in outmigration due to an increase in the relative real wage in comparison to the United States. The fall of the emigration of workers could, thereby, strengthen the potential electorate of the socialist party. To account for this explanation, I collected new detailed data on outmigration from the port of Hamburg to the United States. In contrast to the data by Khoudour-Casteras (2008), the data published by the Statistisches Bureau der Deputation für direkte Steuern (1872-1895) includes information on the sector in which the migrants worked. This source does not cover the complete outmigration, but still provides some tentative evidence on the sectoral structure of migrants in this period.

Analyzing this data, I can check whether and how the sectoral composition of emigrants changes over time. The hypothesis following Khoudour-Casteras (2008) predicts that the introduction of social insurance is associated with a decline in out-migrating workers. While I find a decrease in the share of migrants employed in the industry sector in Figure B3, this decline starts already in 1880 and stagnates after the introduction of social insurance. Also, the absolute number of out-migrating industry workers stagnates.

2.6 Conclusion

In this paper, I show that Bismarck was not successful with his famous policies of *Zuckerbrot und Peitsche* (carrot and stick) to reduce the electoral support for the socialist party. On the

contrary, I find that the socialist party was particularly successful in constituencies with a higher share of newly insured workers by the public health insurance in (i) convincing new voters and (ii) distributing the illegal newspaper of their Marxist wing. Empirically, I rely on a difference-in-differences and shift-share approach exploiting different levels of treatment intensity for the social insurance policies and anti-socialist laws. To address concerns regarding my key identification assumption, I show that industry sectors with a low share of ex-ante insured workers show the strongest positive correlation with vote shares for the socialist party after the introduction of the social insurance. I interpret this result as evidence that indeed, the introduction of health insurance is the key channel. As the main mechanism, I argue that the implementation of the health insurance had a loophole – namely voluntary health funds – that was used by the socialist party as a new organizational platform and thereby became attractive for workers despite relatively higher costs.

However, this paper does not analyse the long-term consequences of Bismarck's policies. Historians have highlighted that the socialist party became more radical during the anti-socialist laws. Due to this increasing isolation, it became later more difficult to form coalitions in Imperial Germany even though the socialist party (and later its successor, the social democratic party) got the majority of votes in general elections (Lidtke, 1966, Chapter XII) . At the same time, the debate about the party position on social security can be seen as a precursor to the revisionism debate and the associated question of the role of reforms vs. revolution starting in the late 1890s. It was precisely this question (in addition to the evaluation of the First World War) that later divided the social democrats into two camps (reform against revolution). Thus, the long-run consequences of Bismarck's policies on electoral success of socialist parties remain an important question to be tackled by future research.

Chapter 3

Migrating from the grain invasion: Trade shocks, labor markets and political polarization in Imperial Germany, 1880-1913

with Richard Bräuer and Wolf-Fabian Hungerland

We study the "grain invasion" of the first globalization (1880-1913) as the historical counterpart to the modern "China shock". Similar to findings on recent trade shocks, we show that trade shocks in agriculture depress the economy of rural counties in Imperial Germany. However, we do not find a corresponding decline in income per capita which we attribute to high levels of labor migration. In line with this result, we also do not observe political polarization as a result of this globalization shock. Overall, our results suggest that the negative and persistent effects of trade shocks in the present are not a universal feature of trade integration. For our analysis, we combine data from three industrial and agricultural censuses on the county-level with national trade data at the product level. For causal identification, we instrument trade exposure with trade exposure for Italy.

3.1 Introduction

The effects of trade integration have long been the subject of major debates in economics. Autor *et al.* (2013) and subsequent papers have added a new argument to these discussions by showing that competition from abroad has long-lasting negative consequences for regions and individuals, depressing local economies, wages, employment rates, worsening workers' health outcomes and increasing political polarization.[18] This has alerted the academic and political debate to the severity of the downside of the recent globalization. However, it remains unclear whether that result is generalizable across world regions and time.

We are the first to show that this result does not transfer to other time periods. We take the case of Imperial Germany in the first globalization (1871-1914) and find that trade shocks also caused regional decline. However, instead of absorbing the losses, workers migrated to booming cities in Germany – the level of migration was four times as high as in the present period. Thus, neither the average income per capita of those remaining in the county, nor their voting behavior or death rates are affected by trade shocks. We suggest to attribute this finding to higher worker mobility, potentially due to the plethora of low skilled jobs available in the industrial center throughout Germany, higher income expectations, or the low level of social security in rural regions.

Germany before WWI offers an ideal context to investigate the effects of trade shocks. During this period, falling transport costs and spreading industrialization created a global economy that resembles many features of today's world economy (Pascali, 2017). The level of trade openness (defined as the share of imports and exports relative to GDP) of 1913 was only reached again 60 years later. Similar to the "China shock" of the present, we also observe large and abrupt changes in trade patterns. The "China shock" of the first globalization was the "grain invasion" (O'Rourke, 1997): Cheap grain from the Americas flooded the European market. Starting from almost zero trade with the USA and Argentina in the 1880s, the annual German import volume dramatically rose over the following decades to more than 400 million Mark (Figure 3.1) This increase captures almost the entire increase in agricultural imports during this period. In this context, we study the labor market, political outcomes and health effects of this shock, using the identification strategy pioneered by Autor *et al.* (2013) for contemporary Chinese imports.

To conduct our analysis, we have digitized sources from the Imperial and the Prussian archives and compile them into a new data set. We combine data from three industrial and agricultural censuses on the county level with national trade data at the product level. From this data, we compute the competitive pressure that each German county faced from abroad and the number of agricultural workers living in these counties. This variation allows us to compare the performance of rural counties with different crop profiles and thus different exposure to world

[18] See Dauth *et al.* (2014); Autor *et al.* (2020); Pierce and Schott (2020); Colantone and Stanig (2018); Dippel *et al.* (2020b)

FIGURE 3.1
TRADE FROM US AND ARGENTINA WITH GERMANY

Notes: The figure shows the development of trade between Germany with the US and Argentina (in Million marks).
Sources: See Section 3.2.

market shocks. However, import competition is likely endogenous: Agricultural producers in Imperial Germany were a powerful political alliance and had a direct impact on trade policy. Thus, domestic firms could, to some extent, choose the level of foreign competition.

To circumvent this problem, we adopt the methodology of Autor *et al.* (2013). We compare the competitive gains of the US and Argentina in the German market with those in Italy. This instrumental variable enables us to determine the strength of the US and Argentinian competition independent of Imperial German trade policy. Italy is a reasonable choice for a comparison market, as it is another newly formed, rapidly industrializing country with no common trade policy with Germany. To our knowledge, there is no other country for which trade data of sufficient detail and quality exists for the first globalization.

Using this methodology, we find substantial negative effects on employment, migration and income. A one standard deviation increase in the trade shock corresponds to a decline in employment growth by 0.5 percentage points per year. In contrast to the current literature, we find that within Germany, workers outmigrate from exposed counties in sizeable numbers, keeping income per capita in the county roughly constant. These findings are robust to the inclusion of several controls, for instance, the level of technology and the share of land owned by big farmers. We confirm the validity of our approach by showing the absence of significant pre-trends. We find neither political responses to trade shocks nor changes in health outcomes, which is consistent with a simple rational choice model of voting. Since trade shocks do not economically

hurt the remaining workers in the hit counties, we do not observe changing voting patterns in these counties.

These results, which contrast with the findings in the present, require some explanation. We view the strong migration response in Imperial Germany as the key economic difference between our time period and contemporary studies. Other studies have also highlighted that workers needed little occupation-specific human capital when taking up factory jobs and that the low skilled formed the majority of migrants (Grant, 2005, p.279ff). We support this interpretation of our results with descriptive evidence using detailed regional statistics on skill-level by migrants' region of origin. In contrast, occupations and specific human capital are crucial for the adjustment process from import competition in the present (Traiberman, 2019): While contemporary workers still switch occupations, they do so within quite narrowly defined fields.

This key economic difference can explain that we cannot find many effects on various welfare measures at the county level: Neither (labor) income per capita, nor death rates respond to trade shocks, in stark contrast to contemporary results. Consequently, we also do not find that counties affected by trade shocks vote for different parties.

This study speaks to two strands of literature. First, we add to the literature on labor market effects and political effects of trade shocks.[19] We contribute by testing whether previous findings are a general feature of trade integration or a specific feature of the current wave of globalization. While the evidence on the migration response is overall mixed for the present period (Greenland *et al.*, 2019; Faber *et al.*, 2019), our results suggest a strong adjustment via migration. In line with the canonical model of Blanchard and Katz (1992), we find a strong migration response, but no income per capita effects in Imperial Germany: While current labor markets cannot reallocate labor between American regions fast enough, the labor markets of Imperial Germany seem to mitigate the trade shocks quickly (similar to US labor markets after WW2).

Second, we also contribute to the economic historiography of the first globalization (O'Rourke and Williamson, 1999; O'Rourke, 1997; Lehmann, 2010; for a recent synthesis see O'Rourke, 2019). Previous studies investigating the effects of the first globalization mostly rest on Heckscher-Ohlin trade theory, long-term outcomes, and cross-country data. By putting together new data on trade volumes, different censuses and tax statistics, we use a more precise measurement for trade shocks by directly linking trade and labor market data. Our results are in line with the long-term predictions based on the neoclassical assumptions of trade models (most notably in this context: migration) used in this literature.[20] Regarding the political effects of trade,

[19] See, among others, Autor *et al.* (2013) for the United States, Dauth *et al.* (2014) for Germany, and Autor *et al.* (2016) for an overview on the effect of trade shocks on labor markets. Dippel *et al.* (2020b), Colantone and Stanig (2018), and Autor *et al.* (2020) study the political consequences.

[20] Comparing our findings with the effect of declining grain prices in two-sector models used in O'Rourke (1997), our effects on (labor) income suggest that the negative effect on income due to migration in cities (by an increase in labor supply) equals the positive effect on real wages due to declining prices for agricultural products.

we revisit the notion that the detrimental effects of the first globalization in agriculture led to radicalization in German politics, at least for the period after 1880. While the pressure in agriculture before 1880 might have led to a political response, we do not find any evidence for a similar reaction during the heydays of the first globalization.

The rest of the paper proceeds as follows: Section 3.2 describes the first globalization in Germany and our way to measure it. Section 3.3 introduces our estimation strategy. In Section 3.4, we present our main findings. Section 3.5 concludes.

3.2 Measuring the first globalization and labor markets in Germany

The first globalization in Germany

After the foundation of Imperial Germany at the end of the German-French war of 1870/71, the new economy rapidly integrated into world markets. The Empire drew a part of its legitimization from overcoming the small borders and market barriers within Germany and relied on the support of nationalistic and liberal middle classes since its inception.

The Americas could enter the world markets in such force because of the rapid expansion of agriculture, railroads and people into the interior of both the US and Argentina. [21] In addition, the technology improvements in the field of transportation, especially steam-ships, reduced trade costs between the Americas and Europe (O'Rourke and Williamson, 1999, Chaper 3; Pascali, 2017). Figure 3.1 shows the resulting development of grain imports into Germany. For our first period until 1895, the level of imports is increasing, but not dramatically so. Imports are accelerating from 1895 onwards, reaching their peak in 1901, a year of massive crop failures.

This economic pressure on the world markets for agricultural products led to fierce debates on trade policies in Imperial Germany starting in the late 1870s. Notably, the Conservatives advocated the protection of the Imperial economy from world markets. The so-called alliance of rye and iron became a powerful (and fragile) interest group pushing for the protection of agricultural and certain industrial sectors through tariffs (Torp, 2010). From this point onwards, tariffs played a major role in Imperial politics, since tariff revenue was crucial for the federal budget, and all tariff changes had to pass the German parliament (Lehmann, 2010, p.153).

Perhaps surprisingly, Germany's franchise was Europe's "most democratic franchise at the time" (Sperber, 1997, p.1), and parliamentary election results are a good indicator for the political

[21] On the economic integration in this period within the US see Donaldson and Hornbeck (2016) as well as Costinot and Donaldson (2016) and within Argentina see Fajgelbaum and Redding (2018).

sentiment, especially on trade issues.[22] The power of the Emperor was maintained through the second chamber of parliament, gerrymandering and his informal influence on the nobility in key bureaucratic positions. The socialist – and often Marxist – Social Democrats were viewed with suspicion and fear by the establishment and represented the leftist alternative to the status quo. Around 1900, the Social Democrats started to oppose protectionist policies.[23] The Protestant liberal and national parties supported the government on this issue at some times and opposed it at others. The Catholic party had no firm stance on tariffs (being split between industrial and agricultural regions). The agrarian and conservative parties advocated for the protection of especially agricultural producers through tariffs and largely supported the Emperor. To the very right, various radically nationalist, folkish and anti-Semitic parties dreamt of a radically different society. Overall, the parliament agreed upon average levels of protectionism in European comparison (Tena-Junguito *et al.*, 2012).

Despite efforts at protectionism, the German economy became more integrated with world markets starting in the 1880s and onwards. This economic integration had different consequences for the industrial and agricultural sector. One main driver of this development was the increase in trade with the rest of the world. For instance, in 1913 almost half (45%) of German imports came from non-European countries, while in 1880 this share was around 30% (Hungerland and Wolf, 2020). More and more agricultural products and raw materials were imported (with an overall increase of around 160% between 1880 and 1913), especially from the US.

The first globalization largely benefited Imperial manufacturing: Germany started to export machinery, chemical, and later pharmaceutical products. Burgeoning global demand created an incentive for a rapid second phase industrialization: Agriculture ceased to employ a majority of all workers around 1880, the year our analysis starts.

Measuring trade exposure

We capture the first globalization quantitatively through measuring trade exposure. We link two technically disjunctive subsets of data: country-industry-level trade data and county-level census data. Their different units of analysis require harmonization. The German trade data compiled by Hungerland and Wolf (2020) contains trade flows in and out of Germany for every modern SITC code for every year between 1880 and 1913.[24] We match the trade data to the agricultural censuses provided by the Prussian statistics (Königlich Statistisches Bureau, 1884, 1895). These censuses include information on cultivation areas for 47 different crops on a county-level. In contrast to the occupation censuses, which only gives us information on the number of workers

[22] Imperial German elections were held directly in single-member constituencies with representatives elected by a majority, following the principle of 'one man, one vote'. Suffrage covered all men above 25 with the exceptions of people under tutelage, in bankruptcy, or on poor relief. However, women were not allowed to vote, so our analysis is limited to the male half of the population.
[23] This stance on trade policy was similar in other European countries (Huberman, 2008).
[24] See Hungerland and Altmeppen (2020) for an in-depth exploration of the SITC to historical data.

in agriculture in general, the agricultural censuses provide us with county-level variation within the agricultural sector.

To link the trade data with the agricultural censuses, we manually build conversion tables between different crops and SITC categories. This procedure leads to 14 harmonized crop categories matched with SITC categories.

We determine which regions face deteriorating market conditions in Germany by spreading the change in net imports for each crop across counties according to the area each county i dedicates to each crop s. We then set the total amount of net imports attributed to each county into relation to the number of workers at the start of each of our two census periods (1880-1895; 1895-1910). Thus, our measure for each county i in year t is

$$\Delta AgriculturalTradeExposure_{it}^{Americas} = \sum_{s} \frac{Area_{ist}}{Area_{st}} \frac{\Delta NetImp_{st}}{Emp_{it}} \tag{3.1}$$

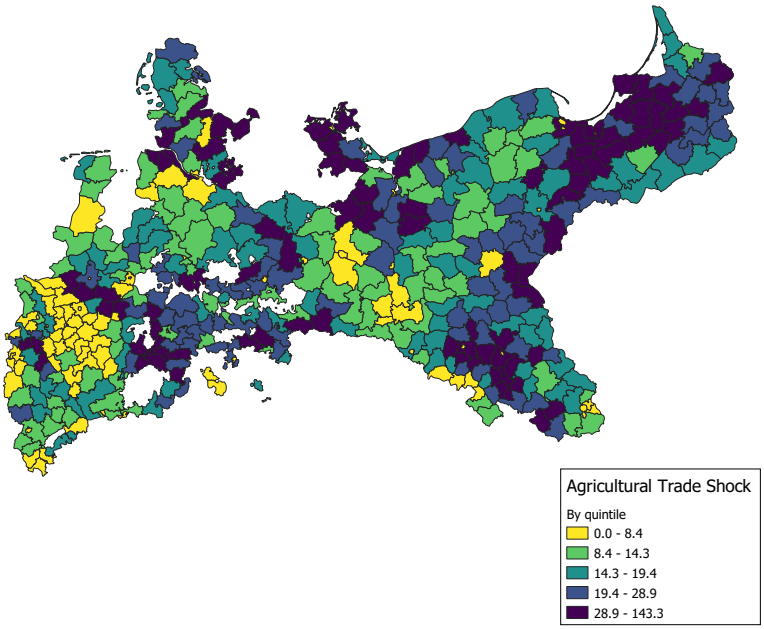
This measure computes the change in the level of trade exposure (relative to the start of the period) as an approximation of the market share foreign products have in the agricultural production in Germany. I.e., if a county faces a change of net-imports of 100 Mark per worker per year, we use this as a measure of how much additional domestic demand is fulfilled by foreign workers.

Series of agriculture outputs have a higher variance than the manufacturing series used today (see Figure 3.1). To exclude the possibility that the random arrival of crop failures drives our results, we take the average of all changes in trade pressure a county experienced throughout our observational periods, instead of just taking the difference between the first and the last value.

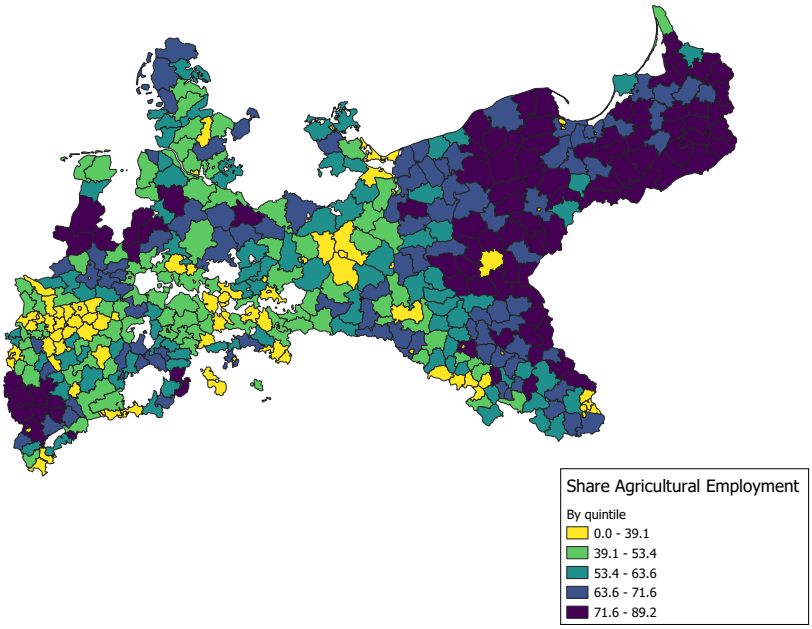
Figure 3.2 (A) shows the counties and their relative average trade exposure for the second period from 1895 to 1910. Darker blue constituencies were more negatively affected. The variation looks very plausible: The eastern part of Prussia is negatively affected by trade shocks. However, even in the rural area, the few cities benefited from the new export opportunities (e.g., Königsberg and Danzig). Crucially for our identification, there is considerable variance between the trade shock and the share of agricultural employment shown in Figure 3.2 (B). These differences reflect the different crops cultivated in the counties. Note, for instance, that in the north-eastern part of Prussia most counties have a very high share of agricultural employment, while the average trade shocks differ substantially. Our identification relies on this kind of within-region variation as we use region fixed effects.

FIGURE 3.2
AGRICULTURAL TRADE SHOCK AND AGRICULTURAL EMPLOYMENT

(A) AVERAGE AGRICULTURAL TRADE SHOCK, 1895-1910



(B) SHARE AGRICULTURAL EMPLOYMENT, 1882



Notes: Panel (A) Trade shock in agriculture between 1895 and 1910. Dark blue constituencies face higher import competition. Panel (B) Share agricultural employment in total employment as of 1882.
Sources: See Section 3.2.

Dependent variables

We study the effects of trade shocks across a large spectrum of outcomes: We consider employment, income and income per capita as economic variables, migration and death rates as demographic variables and voting for radical political parties as a political outcome.[25]

We define employment growth as the average yearly growth in employment. Here, we rely on the three occupational censuses conducted in 1882, 1895, and 1907 as our main source (Kaiserliches Statistisches Amt, 1884, 1898, 1909).

By using the rich information in income tax statistics (Königlich Statistisches Bureau, 1892-1911) and additional sources, we calculate yearly income and income per capita growth starting in 1891.[26] The income tax statistics are only available on the level of districts (a unit higher than counties). These statistics differentiate between income taxes paid by tax units living in urban and rural areas. We link this additional information with census data on the urban and rural population on a county-level. Thereby, we distribute the income taxes paid by the urban population to a county based on its share in urban population within one district. This procedure goes along with the assumption that the average income of an urban and rural tax unit is the same within one district. Alternative measures like wages are only incomplete on a regional level for this period.

To measure migration, a crucial potential adjustment mechanism for trade shocks, we use the difference between the reported "natural" population growth based on births and deaths for each year and the actual population growth between two censuses. Again, we are interested in average yearly changes – in this case relative to the initial population. Thereby, we can decompose total population growth over three decades into its components migration and natural population growth. One caveat is that we do not know where the migrants went to or came from on the county level.

To measure the political consequences of the first globalization, we rely on election data provided by Caramani (2004). To analyze the political effects of the trade shock, we focus on the political left – the back then socialist social democratic party –, numerous radical right-wing, often anti-Semitic parties, and the conservative party – the stronghold of protectionism.[27]

[25] Descriptive statistics are provided in Table C1. We present the spatial patterns of our dependent variables in Appendix Figures C1, C2, C3, C4.
[26] Total income is the sum of taxed and tax-exempt income. We estimate tax-exempt income following Hoffmann and Müller (1959), who produced the first national income series for Germany between 1851 and 1957. We deflate non-filer average income estimated by the Statistisches Reichsamt (1932) for the year 1913 using the wage index for average gross wages in the industrial and agricultural sector from 1870 to 1914 from Kuczynski (1947). For more information on the calculation of the reference total income, see Appendix C in Bartels *et al.* (2019).
[27] To account for potential effects on deaths, we take the data directly from the Prussian censuses provided by Galloway (2007).

Together our data allows us to examine the effect of trade shocks on our dependent variables for two periods: one from 1880 to 1895 and the second from 1895 to 1910. In case one variable is not available for these particular years, we choose the closest year for which it is available, which is at maximum three years away.

3.3 Empirical strategy

We now turn to the empirical analysis. The principle identification strategy has often been used in similar exercises. We thus refer the interested reader to Autor et al. (2013) and Dauth et al. (2014) for more details on the methodology. This literature uses economic competition from China to other similar countries as a way to identify exogenous economic supply shocks and their effects on labor markets and exploits the variation in initial industry specialization at the beginning of the rise of China and its membership in the WTO. In parallel, we analyze the supply shock from the "grain invasion" coming from the Americas and exploit variation in initial agricultural specialization in Prussia at the beginning of the rise of the Americas as an exporter of agricultural products.

To isolate the foreign and plausibly exogenous component of such shocks, we compare the shocks hitting Germany to those hitting Italy, another recently formed and industrializing country during this period. To construct the instrument, we use the Italian trade data made accessible by Federico *et al.* (2011). More specifically, we only include the growing pressure coming from the US and Argentina, the main global competitors for European agriculture.

The trade pattern in agricultural products of Italy to the US and Argentina is similar to the development of Germany. Comparing Figure 3.1 and C5 shows this graphically. While there is no upward trend in the first period and the trade volume on a low level, this pattern dramatically changes in the second period with a high increase in imports from the US and Argentina to the end of the second period.

Apart from data availability, we consider Italy to be a good proxy for the German Empire because it is in a similar situation: Both states imported their raw produce from the world market instead of their colonies and were comprised of industrializing boom-regions and rural backwaters. Moreover, Italy's industrial centers in northern Italy were connected to the world market via Genoa, while the German industrial infrastructure was geared towards the Rhine and Hamburg. Thus, there is no mechanical reason to expect a correlation in trade flows.

These considerations lead to the following instrumental variable that we construct for every county in Prussia:

$$\Delta AgriculturalTradeExposure_{it}^{Americas} = \sum_{s} \frac{Area_{ist}}{Area_{st}} \frac{\Delta NetImpItaly_{st}}{Emp_{it}} \tag{3.2}$$

We use this IV in the following specification to instrument the changes in agricultural trade exposure.

$$\Delta Y_{it} = \beta_0 + \beta_1 \Delta AgriculturalTradeExposure_{it} Americas + X_{it}^{'} \beta_2 + \epsilon_{it} \tag{3.3}$$

In this model, we regress the change of county-level outcome (e.g. employment, income or migration) between t and $t+1$ (ΔY_{it}) on changes in average net exposure with additional controls for start-of-period variables X_{it}. In addition, we use region fixed effects.[28] Summarizing, the logic behind this strategy is that the "grain invasion" induced a supply shock in Europe, and we aim to isolate this supply shock by instrumenting with the grain shock in Italy.

The quality of our instruments crucially depends on three conditions: First, our instrument should be able to explain the change in trade shock to avoid a weak instrument problem. Second, there should be no strong direct links between Italian and German competitiveness and other supply and demand shocks. Third, for the exclusion restriction to hold, the rising market share of the Americas in Italy cannot directly affect German agriculture. We discuss the validity of these assumptions in the next section.

3.4 Results

Main Results

We analyze the effect of trade shocks during the first globalization in Imperial Germany. For current trade shocks, the literature has found long-lasting negative welfare effects for regions and workers. In Imperial Germany, we find that trade shocks decrease the size of the local economy of hit counties but do not affect income per capita, political polarization or health outcomes.

Table 3.1 reports the effects of trade shocks on economic indicators in detail.[29] The number of jobs is a first indicator of general economic conditions in a county. Trade shocks decrease employment growth in a county (panel 1 column 1-3). A trade shock of one standard deviation of imports (15 Mark worth of imports per worker) decreases employment growth by roughly 0.5 percentage points per year. Some rural counties experience trade shocks of 100 Mark per year,

[28] As in the empirical analysis of trade shocks for the present, we construct four regions. The first region (east) consists of the provinces Silesia, Pomerania, East Prussia, West Prussia, and Poznan. The second region (west) consists of the provinces Westphalia, Rhine Province, and Hesse. The third region (north) consists of the provinces Schleswig-Holstein, and Hanover. The fourth region (center) consists of the provinces Brandenburg, and Saxony.

[29] See Table C2 for the OLS results.

decreasing their growth drastically. This picture does not change substantially when including regional fixed effects in column 2.

To further confirm the effects, we employ a set of control variables. We control for the share of land ownership in large estates provided by Galloway (2007). Historians (e.g., Bade, 1980) highlight the role of land distribution for migration decisions.[30] Additionally, we control for the distance to the largest city, which makes it harder to emigrate. Lastly, we control for the technological sophistication with the amount of horsepower installed in the county coming from Prussian statistics (Königlich Statistisches Bureau, 1878). While the technological sophistication of the county is a powerful predictor of a county's employment growth – as expected – none of these controls affect our estimate substantially (as shown in column 3).

Italy's trade exposure is a good predictor for Germany's trade exposure: The F-statistics for excluded instruments is around 100 regardless of specification. The first-stage results in Table C3 show robust and strong effects for our instrument throughout.

Trade shocks induced workers to emigrate from a hit county in sizeable numbers: An increase in the average trade shock by one standard deviation corresponds to roughly 0.1 percentage points of population decline every year (panel 1 column 4-6). The average yearly net migration rate was -0.52. Again, these effects remain stable throughout the different specifications.

This large emigration response marks the first difference between the effects of trade shocks 1900 and 2000. Comparing the level of migration between the first and second globalization reveals strong differences: The difference in employment between the highest shocked quartile and the average shock in the US for the present amounts to 8.2 log points for 25 years (Faber *et al.*, 2019, p.37). Redoing this exercise for the first globalization between 1895 and 1910 yields a difference of almost 20 log points (Table C4).[31] Adjusting for the different lengths of the periods, the level of migration was four times as high during the first globalization.

A closer look at the migration within Prussia provided by Kaiserliches Statistisches Amt (1910) explains who actually migrated (see Table C5). Low-skilled immigrants from rural regions faced good employment prospects in urban centers and made up a large part of the urban population and employees.[32] For instance, more than 30% of all workers in industrializing Brandenburg (including Berlin) were immigrants. We observe similar patterns in other more industrialized provinces like Westphalia and the Rhine province. The jobs these immigrants undertook were of-

[30] More specifically, Bade (1980, p.288-290) stresses that the distribution of land was more equal in the western provinces and links the unequal distribution in the east to more emigration. Note that we capture this general difference by including region fixed effects, but improve precision by including the variable for each county.
[31] Given the high population growth in general, we can also look at population growth driven by migration. Here, we find a difference of 20 log points between the first and fourth quartiles. Unfortunately, it is not possible to exclude "natural" growth from the employment variable. Adjusting for the different length of the periods reveals that migration was almost 70% higher during the first globalization.
[32] The census allows us to define low-skilled migration as factory workers, wage workers, day laborers, and apprentices (Kaiserliches Statistisches Amt, 1910, p.1).

TABLE 3.1
EFFECT OF TRADE SHOCK ON EMPLOYMENT, MIGRATION, AND INCOME (2SLS)

	(1)	(2)	(3)	(4)	(5)	(6)
	Panel 1					
	Employment Growth			Migration Growth		
Shock agriculture	-0.032***	-0.031***	-0.030***	-0.012***	-0.012***	-0.012***
	(0.005)	(0.004)	(0.005)	(0.002)	(0.002)	(0.003)
% Emp agriculture	-0.006	-0.002	0.002	-0.024***	-0.023***	-0.023***
	(0.004)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)
% Land Big Farms			0.001			-0.001
			(0.004)			(0.002)
Distance to large city			0.000			-0.001
			(0.002)			(0.001)
Horsepower p.w.			2.204***			-0.250
			(0.714)			(0.522)
	Panel 2					
	Income Growth			Income p.c. Growth		
Shock agriculture	-0.017***	-0.016***	-0.021***	0.001	0.001	-0.002
	(0.004)	(0.005)	(0.007)	(0.003)	(0.003)	(0.004)
% Emp agriculture	-0.038***	-0.033***	-0.028***	-0.010***	-0.007***	-0.006***
	(0.004)	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)
% Land Big Farms			0.008			0.003
			(0.005)			(0.003)
Distance to large city			-0.002			-0.002
			(0.002)			(0.001)
Horsepower p.w.			1.419			-0.707**
			(0.877)			(0.307)
F-Stat excl. inst.	120.30	118.25	116.98	120.30	118.25	116.98
Region FE		✓	✓		✓	✓
Observations	449	449	449	449	449	449

Notes: Standard errors, clustered at the district level, in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
Sources: See Section 3.2.

ten tough and paid comparatively low wages. Nevertheless, they gave unemployed rural workers a viable alternative to the countryside that many of them took once trade shocks had worsened their original employment.

This interpretation is supported further by regressing trade shocks on the usual indicators of broad welfare decline in the literature on trade shocks: Of these, we focus on total income, income per capita, polarized/radical voting behavior, and excess deaths. Panel 2 of Table 3.1 reports the effect of trade shocks on income. Importantly, while total income declines, it does so in line with emigration: Income per capita is stable in shocked counties (see column 4-6).[33]

The same holds for the voting share of radical left or right parties (from the perspective of the times) and death rates. This finding is not surprising, since the literature argues typically that current trade shocks lead to political polarization and adverse health outcomes through the adverse labor market effects (Autor *et al.*, 2020; Dippel *et al.*, 2020b). The results in Table 3.2 show no significant impact of the trade shock on radical parties on both sides of the political spectrum. Note that we also find no effect of the trade shock on the protectionist conservative party or political mobilization in panel 2 of Table 3.2. Similarly, we do not see an increase in death rates due to the trade shock in Table C7.

Table 3.2
Effect of Trade Shock on Political Outcomes (2SLS)

	(1)	(2)	(3)	(4)	(5)	(6)
	Panel 1					
	Socialist			Nationalist		
Shock agriculture	-0.045	-0.045	0.001	-0.051	-0.051	-0.027
	(0.066)	(0.066)	(0.046)	(0.057)	(0.057)	(0.063)
F-Stat excl. inst.	95.06	95.06	91.33	94.13	94.13	87.47
	Panel 2					
	Conservatives			Turnout		
Shock agriculture	0.095	0.095	-0.020	0.015	0.015	0.020
	(0.084)	(0.084)	(0.083)	(0.027)	(0.027)	(0.034)
F-Stat excl. inst.	110.47	110.47	103.93	95.03	95.03	84.39
Region FE		✓	✓		✓	✓
Further Controls			✓			✓
Observations	225	225	225	225	225	225

Notes: Standard errors, clustered at the district level, in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Further controls include: share employment in agriculture, distance to large city, share of land owned by large farm owners (more than 50 hectare), horsepower per worker. We control for the initial share of the party throughout all specifications.
Sources: See Section 3.2.

[33] One potential concern with our income variable is that it also includes capital incomes. To rule out this concern, we exclude income from capital. Our results remain almost identical (see Table C6).

Threats to identification

Before putting our results in a broader context, we address several threats to our identification. While we instrument imports into Germany with imports into Italy, this alone need not be sufficient to get exogenous variation. Still, threats to identification remain:

First, shocks could disproportionally hit those parts of the economy already in decline. Our trade measures are defined within the declining agricultural sector, but we compare different segments within agriculture. Thus, if, e.g., wheat farming inherently experiences less growth than vegetable farming and is additionally hit by trade shocks, we would wrongly attribute the decline of wheat farming to international trade. To deal with this issue, we compare the growth performance of regions with certain crops before and after the trade shock. Figure C7 reports the results of this exercise, which follows Goldsmith-Pinkham *et al.* (2020). It shows that the growth performance of regions with a high share of wheat, oat, barley, oil fruits, and fodder worsened significantly in the shock period, compared to the 1880-1895 time period. Conversely, regions relying on rye, potatoes, or grazing grew faster in the shock period (1895-1910) than before. This pattern is well explained by our trade shock, which consists mainly of wheat, oil fruits, and forage maize imports into Germany.

Second, farmers in Imperial Germany might anticipate imports and move away from contested products before the actual trade shock. This would bias the results towards zero since our measure of import competition would no longer capture the actual pressure faced by regions. To account for this effect, we use the original distribution of cropland in 1883 also for our shock measure from 1895 onwards. We can safely discount the possibility that farmers could predict trade shock 20 years in advance. Table C8 reports that the results for our major regressions do not change when using this technique. We thus are confident that our main results are impervious to this critique.

Third, to further exclude the possibility that our trade shock is correlated with the pre-shock performance of economic activities, we only take the difference in our main outcome variables between the first and second period into account. This procedure controls for the pre-trend trajectories of our dependent variables and mitigates the concern that trade shocks might be correlated with regional equilibrium growth paths. The results of this exercise in Table 3.3 overall support our main findings. While the coefficients for employment and migration growth are smaller in magnitude, they still are significant. The effect on income growth turns insignificant. Note, however, that the income variable is only available starting in 1891. Thus, we only cover only a small portion of our pre-period.

Fourth, to account for the possibility that counties do not represent local labor markets, we redo the analysis on the level of electoral constituencies, which often consist of two or three counties. The results in Table C9 confirm our previous findings. If anything, the coefficients are somewhat larger. Importantly, our result that trade shocks affect migration and income, but not income per capita stays qualitatively unchanged.

Table 3.3
Pre-Trend: Effect of Trade on Second Difference (2SLS)

	Employment (1)	Migration (2)	Income (3)	Income p.c. (4)
Shock agriculture	-0.015***	-0.004*	-0.004	0.001
	(0.004)	(0.002)	(0.005)	(0.005)
F-Stat excl. inst.	116.49	116.49	116.49	116.49
Region FE	✓	✓	✓	✓
Further Controls	✓	✓	✓	✓
Observations	449	449	449	449

Notes: Standard errors, clustered at the district level, in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Further controls include: distance to large city, share of land owned by large farm owners (more than 50 hectare), horsepower per worker, employment share in agriculture. The dependent variable is defined as the difference between yearly growth in the different dependent variable in the second period and first period.
Sources: See Section 3.2.

3.5 Conclusion

Our paper analyzes the economic, political, and health effects of trade shocks during the first globalization in Imperial Germany. We find that trade shocks hurt the local economies affected, just as in the present. However, trade shocks did not cause the long-run losses in per capita income, political stability, and health outcomes that have been observed today.

We attribute this difference to the large effects of trade shocks on migration within Germany: Instead of bearing income losses, rural workers from affected counties moved to the booming cities, where low-paid low-skill employment was available for a large number of new workers. At the same time, they bore the cost of movement. Still, the possibility to easily switch to new employment was likely a key reason for the relatively benign outcomes of the first globalization in rural German counties. There might be other reasons as well: low home-ownership and a weak social safety net might also play a role besides low switching costs. Overall, the level of migration was considerably higher in comparison to the second globalization in the US.

We use the large influx of agricultural products from the recently developed interior of the Americas as a natural experiment, shocking German counties specialized in the production of wheat, fodder, or oil fruits, while sparing those that focused on, e.g., animal husbandry or rye.

Our results highlight that the effect of trade on regional and personal outcomes are more contingent than the current literature suggests. While large adverse negative effects have been found in several developed economies, we argue that these depend on the level of migration as a crucial adjustment mechanism. This does not mean, however, that the welfare and distributional consequences of the trade shock in our setting are clear. Still, from an economic policy perspective, these findings show the importance of labor market mobility as an adjustment mechanism for

trade shocks. For current economic policy, it, therefore, seems important to better understand the frictions in labor markets and specifically mobility, whether in the area of labor supply (e.g., the role of specific human capital or income expectations) or labor demand (e.g., availability of jobs).

Chapter 4

Weber revisited: The Protestant Ethic and the Spirit of Nationalism

with Iris Wohnsiedler and Nikolaus Wolf

We revisit Max Weber's hypothesis on the role of Protestantism for economic development. We show that nationalism is crucial to both, the interpretation of Weber's Protestant Ethic and empirical tests thereof. For late nineteenth-century Prussia we reject Weber's suggestion that Protestantism mattered due to an "ascetic compulsion to save". Moreover, we find that income levels, savings, and literacy rates differed between Germans and Poles, not between Protestants and Catholics, using pooled OLS and IV regressions. We suggest that this result is due to anti-Polish discrimination.

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4.1 Introduction

Economists increasingly acknowledge the role of "culture" for economic development, related to a new, broader approach in economics to human behavior and decision making. One of the most influential ideas today stems from Max Weber's work more than one century ago. Weber (1904, 1905) famously hypothesized that a specific Protestant work ethic fostered modern economic development due to a "compulsion to save". He motivated this with some statistical evidence on differences between Protestants and Catholics in Baden around 1900 and used anecdotal evidence to suggest a much more general relationship.

In this paper, we revisit Weber's hypothesis and the evidence on it for nineteenth-century Germany. We argue that the "common interpretation" (Delacroix and Nielsen, 2001) of Weber has often missed his own argument on saving behavior as the key mechanism. Moreover, it missed Weber's nationalist and anti-Polish bias. Weber wrote in a context where religious differences (between Catholics and Protestants) overlapped with ethno-national differences (between Poles and Germans), and he was well aware of this. Max Weber was a passionate German nationalist, and his writing, including the "Protestant Ethic" (PE), should also be understood as a contribution to political education of the German public (Barbalet, 2008). In fact, the PE should be seen as much as an "intervention into German political concerns than an account of the emergence of capitalism" (Scott, 2009, p.903). We show empirically that we need to modify the so-called "common interpretation" twofold: with a focus on savings as a mechanism by which religion might have affected economic outcomes and by controlling for differences between ethnic groups as a possibly crucial confounding factor.

Prussia in the nineteenth-century provides us with rich variation for a new test of Weber's PE. Starting with basic descriptive evidence we show that income levels, savings and literacy rates across Prussia after 1870 are more strongly correlated with differences between ethnic groups than with those between religious denominations. Simple ordinary least squares (OLS) regressions suggest that ethnicity is likely to be a crucial factor in explaining the large variation in outcome variables across the German Empire. To provide causal evidence on the role of culture for development, we follow Acemoglu (2009) and distinguish between proximate and fundamental causes of economic growth. We focus on savings and literacy rates as possible proximate causes of growth, and on religion capturing aspects of culture as a potential fundamental cause of growth. We then use an an instrumental variable (IV) approach to test for causality: income levels, savings, and literacy rates did not differ between Protestants and Catholics, but differed between Germans and Poles. Let us explain in more detail, how we come to this conclusion.

In a first step of our empirical analysis we revisit the evidence on the underlying mechanism, motivated by reading again Weber's original text. Weber suggested that Protestantism might have led to an "accumulation of capital through ascetic compulsion to save" (Weber, 1905, p.191). This idea has recently been formalized by Alaoui and Sandroni (2018). To test for it more directly, we use a recent data-set from Lehmann-Hasemeyer and Wahl (2017) on savings

per capita and the number of savings banks for all Prussian counties, available for the years 1875, 1882, 1888, 1898 and 1904. To test for a causal effect of Protestantism on savings, we use an IV based on religious denominations as of 1624, following Spenkuch (2017). The IV is constructed by regressing Protestant in 1624 on predictors of rulers' choices, as identified by the previous literature, notably Cantoni (2012) and Rubin (2014). The residuals are used as instrument for the share of Protestants after 1870. We show that the instrument is strong and robust to violations of the exclusion restriction. We find no significant effect of Protestantism on savings, neither in OLS nor IV. This finding is also robust to variations in the sample, alternative specifications, the inclusion of income per capita as a regressor, and a distinction between Lutherans and Reformed Protestants. In contrast, we find that ethnic differences as measured by the share of German speakers are strongly correlated with savings per capita.

Next, we test for the idea that Protestantism mattered not due to a change in attitudes towards work and consumption, but because it fostered literacy as argued by Becker and Woessmann (2009). We first provide new descriptive evidence on the difference in literacy rates among Protestants and Catholics at the county level from a historical cross-table, which has been largely neglected in the literature. We show that at the county level literacy rates among Catholics are nearly identical to literacy rates among Protestants. The only exception to this are counties with a substantial share of Polish population. Second, we test for a causal effect of religion on literacy, using again the IV-approach by Spenkuch (2017). We find that Protestantism had no significant effect. The coefficient on ethnic differences is much larger and statistically and economically significant.

This set of results leaves us with a new puzzle. Apparently, religious differences help very little to understand the large variation in terms of economic development across nineteenth-century Imperial Germany, respectively Prussia. A naive interpretation of Weber's PE can, therefore, be clearly refuted in our context. Also, the more sophisticated interpretation of a role of Protestantism via literacy rates finds no support in the data. We note that this does not rule out that ascetic Protestantism had causal effects centuries before, which indirectly may have affected Catholics and indeed secularization. An alternative interpretation could place Weber's argument in the context of technological change or test for altogether different mechanisms on how the Protestant Reformation might have caused economic development (Cantoni *et al.*, 2018). But our findings suggest that differences in religion between Catholics and Protestants (encompassing Lutherans) had little persistence, hence they cannot be considered to be "fundamentals" without further qualification. Instead, our evidence highlights the enormous differences between ethnic groups (Germans and Poles) that existed around 1900 in Prussia, dwarfing any differences between Protestants and Catholics. This new result is in line with most of the historical literature on the German Empire, which stresses rising national tensions and abating religious conflict after the end of the *Kulturkampf* (Wehler, 2006).

In the last part of our paper, we discuss this relationship between ethnicity and economic outcomes and suggest some lines for further research. This issue is important, not only due to very high correlation between differences in economic outcomes, ethnic groups, and religion but

also because Weber himself considered ethnicity as crucial. In his earlier writing he attributed differences in economic outcomes between Germans and Poles to racial differences (Weber, 1895), and actively supported a stronger Germanization of the eastern parts of Germany. Thus Weber's nationalist position not only puts a different light on the PE (as discussed in recent research in sociology) but also offers a contemporary point of reference for explaining these differences, namely German discrimination. We document that after 1871 the German majority increasingly discriminated against the Polish minority in terms of language and education policy, and also when it came to access to public offices and policies of land redistribution. Poles might have reacted to this with attempts to create substitutes, such as education outside of state schools or the emergence of Polish land and credit cooperatives. More research along these lines is needed to understand, to what extent Germanization can account for the observed large differences in incomes, savings and literacy rates, which we documented above.

Our paper contributes to several strands of research. First, we add to the growing literature on the role of culture, and specifically religion for economic development. One part of the literature has focused on theoretical arguments, how culture can affect preferences, behavior, and outcomes, including Bénabou and Tirole (2004, 2006, 2011), Tabellini (2008), Doepke and Zilibotti (2008), and Alaoui and Sandroni (2018). To some extent, this literature can be seen as an attempt to rationalize the behavioral consequences of cultural attitudes, such as Weber's proposition that ascetic Protestantism leads to increased savings and wealth accumulation. Another part of that literature seeks to test for the role of culture for economic development empirically, see Iannaccone (1998), Barro and McCleary (2003), Becker and Woessmann (2009), Cantoni (2015), Spenkuch (2017), and Becker *et al.* (2016) for a review. Our first contribution is a focus on savings behavior as a mechanism that links Protestantism and economic development. This mechanism is of importance as it is at the core of Weber's PE and also central to much of the recent theoretical literature, including Alaoui and Sandroni (2018). We can clearly reject this hypothesis for late nineteenth-century Prussia. Another contribution is that we reconsider a prominent alternative hypothesis, suggested by Becker and Woessmann (2009): that Protestantism mattered for development via literacy rates. Again, we can reject this hypothesis in our context. We provide new direct evidence at the county-level that literacy rates between Protestants and Catholics in 1871 did not differ. Instead, we show that literacy rates differed a lot between ethnic groups, which the previous literature did not consider.

Next, we show that nationalism and ethnic conflict is key for both, an understanding of Weber's PE and an understanding of differences in terms of economic development. The former is well known in the sociological and historical literature on Weber and the PE (e.g. Mommsen 2004; Zimmerman 2006; Barbalet 2008), but has been largely ignored in the economic literature on the topic. Our contribution here is to remind economists about the context in which Weber was writing (Margo, 2017), and to show empirically that ethnic differences are indeed a crucial confounding factor. While our data is for Germany in the late nineteenth-century, we think that religious and ethnic differences may be more systematically related. Our findings suggest that the previous literature may have overestimated the role of religion by failing to take ethnic differences into account.

Finally, we contribute to the literature on the long-run economic development of Germany (Broadberry and Burhop 2007; Grant 2005; Wolf 2009; Hornung 2015; Bartels 2019). We document that the German Empire was characterized by fragmentation between religious groups, between different ethnic groups, territories with different historical legacies, a growing rural-urban divide and growing economic inequality. The economic development of Germany before WWI was, to some extent, shaped by these lines of fragmentation (e.g., Wolf 2009). Recently, Cinnirella and Schueler (2016) have shown that fragmentation along ethnolinguistic lines mattered for education policies. Our empirical results suggest that they mattered more generally for differences in economic development.

The rest of the paper is organized as follows. In section 4.2 we briefly review Weber's essay on the PE and the "common interpretation" of it that has served as a workhorse for much of the recent work on the subject. Section 4.3 provides historical background on economic development across late nineteenth-century Germany. In section 4.4 we state our main hypotheses and discuss our empirical strategy to test for causality. Section 4.5 contains our main results on income, savings and literacy rates as well as several robustness checks. In section 4.6 we discuss how discriminatory policies might account for the observed role of ethnic differences, before we conclude in section 4.7.

4.2 Weber's Protestant Ethic and the "common interpretation"

Weber's work "The Protestant Ethic and the Spirit of Capitalism" is a founding text for sociology and continues to be widely discussed, also in neighboring disciplines such as history and economics. The text was first published in two parts in 1904 and 1905 and immediately received much attention, praise, and critique. In 1920 Weber published a revised version that incorporated some of the earlier discussion and is the basis for all following debates as well as translations (Lehmann and Roth, 1995). The interpretation of the PE has changed over time, which is to some extent due to the complexity but also the ambiguity of Weber's arguments (MacKinnon, 1995). We cannot even superficially review the entire discussion, but focus instead on some aspects, which have been taken up in the more recent empirical (and mostly economic) literature.

A convenient starting point is the "common interpretation" of Weber's hypothesis as formulated by Delacroix and Nielsen (2001, p.511) and Nolan and Lenski (2014), which is central for the recent empirical literature on the subject. Given the vast literature on the PE and its appropriate exegesis, these authors condensed a simplified, yet testable summary as follows: 1) The Protestant Reformation fostered new attitudes, 2) the new attitudes (the PE) affected behaviors, and 3) the new attitudes and behaviors favored economic development and contributed to Industrialization around the world. It is this interpretation of Weber's PE, which has been recently tested and discussed in the economics literature.

The "common interpretation" remains unspecific about the possible mechanism that might lead from a change in attitude to a change in behavior and finally economic development. This is due to the fact that Weber himself was eager to provide many caveats. At the beginning of his essay, he pointed out that there might be an issue of reverse causation, namely that it was the richer parts of the Empire, notably rich cities that first adopted Protestant ideas in the sixteenth century. He also suggested that a higher income of Protestants around the time of his writing could result from various historical factors and in turn contribute to observed differences in economic behavior. Towards the end of his essay, he suggested that a thorough analysis would have to examine how a Protestant work ethic may have affected the organization of societies and their political institutions as well as the formation of modern science and technology, among other things. But at the heart of his essay stand clear statements about one particular underlying mechanism. In Weber's view, the formation of a new attitude (ascetic Protestantism) leads to a change in behavior, namely a limitation of consumption and increased economic activity, so that "the inevitable practical result is obvious: accumulation of capital through ascetic compulsion to save" (Weber, 1905, p.191).[34]

Influential empirical contributions include Delacroix and Nielsen (2001), Barro and McCleary (2003), Becker and Woessmann (2009), Cantoni (2015), and Spenkuch (2017). Delacroix and Nielsen (2001) explore the correlation between cross-country evidence on measures of the share of Catholics and Protestants on various indicators for economic development, including measures of wealth, savings bank deposits per capita, extent of the railroad network and others. Overall, they find only limited empirical support for the "common interpretation". Barro and McCleary (2003) use a global cross-country data-set to test for a relationship between religious beliefs, church attendance and economic outcomes. They employ panel-data estimation and find that religious beliefs matter, notably belief in Hell, after controlling for church attendance and considering reverse causation. Becker and Woessmann (2009) use data on Prussian counties from the late nineteenth-century, the time when Weber was writing, and find a causal link from Protestantism to economic development. However, they argue that this can be fully explained by higher literacy rates among Protestants rather than any effect of religious attitudes. Cantoni (2015) uses panel data on city growth as an indicator for economic development across the Holy Roman Empire for the centuries 1300 to 1900 and finds no effects of differences in confessions. Spenkuch (2017) uses microdata from the Socio-Economic Panel (SOEP) on Germany today to test for differences between Protestants and Catholics in terms of their attitudes, behaviors, and outcomes. He finds some evidence suggesting that Protestants work longer hours due to different work-ethic as captured in specific survey questions.

Why does the "common interpretation" and most of the empirical work on it neglect Weber's own and very sharp distinction between Lutheranism and ascetic Protestantism such as Calvinism? Weber stressed the role of "calling", which is the religious and irrational underpinning of

[34] In the translation by Talcott Parsons see Weber (1930, p.172). In line with this reasoning, Dohmen *et al.* (2018) find a strong reduced form relation between patience and economic development using a large global data-set on time preferences of individuals.

the PE. Weber argued that for Luther this concept of calling remained traditionalistic while for Calvinists it implied a restless striving for worldly success.[35] The common failure to distinguish between Lutheranism and other Protestant denominations in the empirical literature is in part to due Weber's own use of evidence in the PE: on the very first page of the PE Weber motivates his work with statistical data on differences in school attendance between Catholics and Protestants in Baden, drawn from his student Martin Offenbacher (1901). Apart from some statistical errors that actually exaggerate these differences (Becker, 1997), the data does not allow us to distinguish between Lutherans and other Protestants. Hence, Weber himself placed his argument in the context of apparently striking differences between Catholics and Protestants – only to elaborate further on the dynamic nature of ascetic Protestantism and implicitly stressing the backwardness of Catholicism.

The "common interpretation" rests on the assumption that Weber's text should be seen as a study in the origins of modern capitalism and economic development. But much of the recent literature on Weber stresses the contemporary context in which he wrote, notably the political dimension of his writings and the place of the PE in relation to his earlier work. Wolfgang Mommsen (2004, p.38), the editor of the collected writings of Max Weber, stated that his inaugural lecture (IL), given at the University of Freiburg in 1895, was the most important document of the political Max Weber until WWI, which is relevant in this context. Under the title "The National State and Economic Policy" (original "Der Nationalstaat und die Volkswirtschaftspolitik"), Weber states that he wants to clarify "the role of physical and psychological racial differences between nationalities in the economic struggle for existence" (Weber, 1895, p.545). His example are the differences in the Prussian province of West Prussia between Poles and Germans. Quoting statistical evidence, he suggests that while Polish peasants live on less fertile ground, they are less prone to out-migrate than German peasants in response to the recent agricultural crisis, resulting in "economic displacement" of Germans by Poles. His hypothesis is that this is rooted in different attitudes between the two nationalities, specifically the "lower requirements about the standard of living [...], which the Slavic race has by its nature or acquired over its past" (Weber, 1895, p.551). Hence, already ten years before the PE, in his IL in 1895, Weber argued that different attitudes can lead to different behavior (in this case migration), and outcomes (the displacement of Germans by Poles). Crucially, he demands that economic policy ought to stop this, because "our state is a national state" (Weber, 1895, p.558). According to Ernst Troeltsch, "die nationale Kraft und Größe" ("the national power and greatness", own translation) was the only "Wertgott" whom Weber unconditionally worshiped (Troeltsch, 1922, p.161). Based on the example of the national conflict between Poles and Germans in the East, for Weber the (Lutheran) Prussian Junkers have failed to provide political leadership. He concluded that the German bourgeoisie is in urgent need of political education (Weber,

[35] As shown in Graf (1995, pp.41ff.), Weber argued here against the contemporary mainstream view defined by the theologian Albrecht Ritschl and his students, who regarded Lutheranism as the cornerstone of the new German nation state. However, while both considered Catholics as traditionalists and culturally inferior, for Weber Lutheranism represented a "deficient form of Protestant religiosity, closer to the level of traditionalist Catholic conduct than to the ethical activity of the Calvinists" (Graf, 1995, p.45).

1895, pp.570-3).[36] Barbalet (2001) drew a line from the arguments Weber made in his IL to the Protestant Ethic. He suggested that in the PE of 1904/1905 Weber took up this challenge first formulated in his Freiburg lecture and provided a solution to the problem of political education: to face the threat to the German nation, in particular the threat of Polonization, Weber argued that the Lutheran German elites needed a new calling, and ascetic Protestantism could provide it.

This reading of Weber's PE is more than a historical footnote, because it affects our priors. It is unclear whether we should expect to find any substantial differences between Protestants and Catholics across nineteenth-century Germany in terms of indicators for economic development. In the next section, we briefly provide some relevant historical background on this.

4.3 Economic development and political tensions in Imperial Germany

The German Empire at the time of the first publication of Weber's PE in 1904 and 1905 was characterized by a dynamically growing economy together with increasing social and political tensions. Between 1850 and 1910 Germany developed from a backward economy into Europe's industrial core.

The war against France in 1870/1871 had led to the formation of a new political entity–the German Empire –which was highly heterogeneous, given its rather small size. The Prussian state had a leading position, representing about 60 percent of the entire population But Prussia itself was heterogeneous, split between dynamic cities such as Berlin and backward agrarian regions, old Prussian territories such as Brandenburg and new ones recently conquered after the Napoleonic Wars such as the Rhineland (1815), Hannover (1866) or Schleswig-Holstein (1866). And most importantly, Prussia experienced conflicts between Catholics and Protestants, and between Germans, Poles, and other national minorities.

Hence, after the formation of the Empire in early 1871, its character as a nation state remained contested and Prussia as the largest and most influential state was the central arena for these public debates. The first line of conflict opened between the Prussian state on one side and the Catholic church and catholic organizations on the other, the *Kulturkampf*. While the conflict between the Catholic Church and state authorities was a European phenomenon at the time, the dispute was especially intense in the German Empire. In Alsace-Lorraine and the east-

[36] Weber maintained his anti-Polish nationalism until the end of his life. In 1899 he resigned from the nationalist Pan-German league because he believed the organization had given in to the interests of agrarian capitalists allowing the influx of cheap Polish migrants instead of fighting the Polish threat (Zimmerman, 2006, p.64). While Weber turned into a critic of German colonialism later in his life, still in 1918 he agitated during a lecture against Polish officials who would dare to enter the contested city of Danzig, and proposed they should be shot (Zimmerman, 2006, p.64).

ern Provinces of Prussia this conflict overlapped with resistance of national minorities against German nationalism, because these minorities were overwhelmingly Catholic. From the Polish perspective this *Kulturkampf* was considered first and foremost an attack on Polish identity (Trzeciakowski, 1970). Here the conflict between state and church was fierce (e.g., the Catholic Archbishop of Gniezno and Poznan Ledochowski was imprisoned 1874-1876) and it coincided with the first steps of repression against the Polish language in primary and secondary schools after 1871 (Knabe, 2000).

The situation changed with Bismarck's political turn in 1878/1879, which ended the *Kulturkampf*, but opened new lines of conflict against Socialists and national minorities accompanied by rising antisemitism and protectionism. The decades between 1880 and the WWI were characterized by swelling German nationalist sentiments, reflected for example in the growth of nationalist *Kriegervereine* (veteran organizations). Their membership increased from 71900 (below 1 percent of the male population) in 1873 to 2.8 million in 1913 (above 8 percent of the male population), more than all trade unions in the Empire (Kersting, 2017). Especially the Poles in the East of Prussia were considered as a threat to the German state and faced harsh oppression. The *Geschäftssprachengesetz* of 1876 and the *Gerichtsverfassungsgesetz* of 1877 prohibited the use of Polish in administration and the judiciary (Wehler, 2006). Under Prussia's new minister for education, Gustav von Gossler, from 1881 onwards there was a wave of new anti-Polish measures in an attempt to stifle the use of Polish language in schools and to reduce the number of Polish teachers. After years of discussion, in 1901 the Prussian state attempted to eliminate the Polish language also from religious education, which immediately triggered a first local school strike in the city of Wrzesnia in 1901. It was followed by a much larger strike in 1906. The Prussian state responded with disciplinary measures and the strike was finally broken, but the tensions about language policy and indeed national identity of Poles and Germans did not abate.

Hence, late nineteenth-century Germany was indeed characterized by fundamental conflicts between religious denominations and nationalities, which intensified after the formation of the German Empire in 1871 and in waves thereafter. To what extent were these conflicts reflected in economic outcomes at the time? Wolf (2009) has shown that language differences mattered much more than religious differences as a barrier to internal trade flows within the Empire between 1885 and 1913. In the next sections we will analyze whether language and religious denominations were systematically related to different levels of economic development, whether this can be interpreted as causal and what might be the mechanisms underneath.

4.4 Hypothesis, data and empirical strategy

In our empirical analysis, we focus on differences in terms of income levels, literacy rates and savings as indicators for economic development across Prussian counties and over time. Specifically, we aim to test the following three hypotheses:

H1: Protestantism had a causal effect on saving behavior.

H2: Protestantism had a causal effect on literacy rates.

H3: Ethnic differences are an omitted variable in the "common interpretation".

We distinguish between proximate and fundamental causes of economic growth (Acemoglu, 2009). Proximate causes include differences in technology, physical capital (as reflected in savings) and human capital (as reflected in literacy rates), fundamental causes include geographical factors (e.g., coal resources), institutions (e.g., property rights) and culture (e.g., a specific work ethic).

To test our first hypothesis, we investigate the causal effect of Protestantism on savings as a specific proximate cause for development that in turn may have affected long-run growth and income levels. To do so, we use regional data on Sparkassen (savings-banks) provided by (Lehmann-Hasemeyer and Wahl, 2017). The first savings-bank was founded in 1778 in Hamburg and intended to serve the benefit of "poor, industrious persons of both sexes, working as servants, day labourers, manual workers, seafarers etc.", in order to give them the opportunity to save money (von Knebel Doeberitz, 1907, p.2). Savings banks combined the functions of deposit banks and credit institutions, whereby they were meant to fulfill their purpose as an institution for the prevention of poverty. In contrast to social insurance provided by the state, saving is left to the free choice of the individual. Thus, data on the deposits in savings-banks allow us to observe the savings behavior of medium- to lower-income groups, who were encouraged to save so they could pay for unforeseen expenses and re-invest the money in their agricultural or craft business, thereby contributing to a better standard of living in the long term (Trende, 1957, p.129).[37]

While capital accumulation in agricultural and commercial credit cooperatives was needed to ensure the supply of credits to the members of the cooperatives, fostering savings was an end in itself for the savings banks (Trende, 1957), corresponding to Weber's emphasis on the importance of savings for economic development. Due to their specific purpose and the focus on broader parts of the population as their customers, we argue that savings-banks are an appropriate institution to study regional differences in savings within Prussia. Additionally, the so-called *Regionalprinzip* (regional principle), according to which it was not possible to open an account if one did not live in that region, ensures that the savings-deposits reflect the propensity to save of a county (Lehmann-Hasemeyer and Wahl, 2017).

[37] We note that our data might imply some bias if higher incomes from entrepreneurial and industrial activity would systematically differ between Protestants and Catholics. While this would require further research, the available evidence suggests that there was no such bias in the Prussian data before 1914. First, the share of income millionaires in the population was not correlated with Protestantism in 1912 (Martin, 1912). Second, using the Prussian tax statistics we find that the share of capital income in total income of a district was not correlated with Protestantism between 1893 and 1910 (Bartels *et al.*, 2019).

Figures 4.1 and 4.2 show the geographic distribution on the savings per capita in 1880 and 1905, which sharply increase during this time. In most regions, except for the eastern provinces, savings per capita are above 100 *Mark* in 1905. Moreover, almost every county had at least one savings bank. Can differences in religious denomination possibly account for these differences in savings? Figure 4.3 shows the geographic distribution of Protestantism in Prussia as of 1900, and suggests that there might indeed be some relation.

The data on savings is available for the years 1875, 1882, 1888, 1898, and 1904. We link this data with the closest available census to calculate savings per capita. We estimate the following specification:

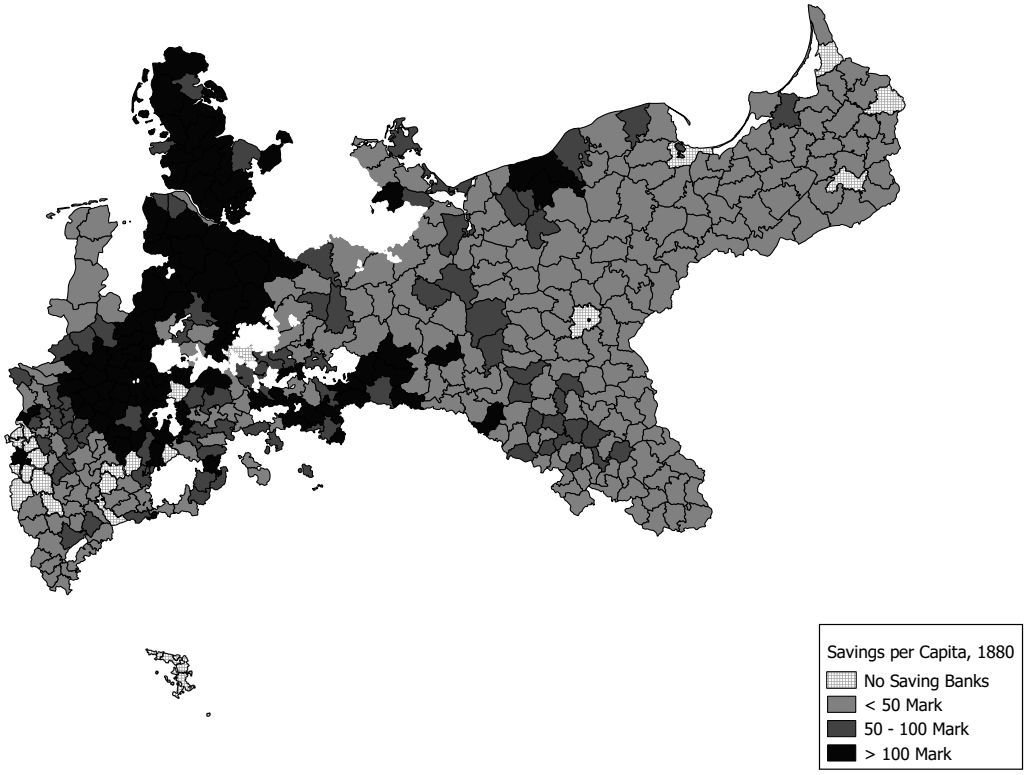
$$Sav_{it} = \alpha_1 + \beta_1 Prot_{it} + X'_{it}\gamma_1 + \epsilon_{1it} \tag{4.1}$$

where Sav_{it} indicates savings per capita in county i and year t, $Prot_{it}$ the share of Protestants, X_{it} time-varying control variables including urban population, the number of savings banks, household size, and share of other religions beside Catholics and Protestants.[38] We prefer pooled OLS regressions because there is only limited time variation in our main independent variable, the share of Protestants. We refer to Table D1 in the Appendix for further information on our data. To control for income per capita, we combine sector-level employment statistics for each county with sector-level national wage data. To account for variations in sector-level wages across counties, we weight our income variable with county-level data for wages of day labourers. If not noted otherwise, we use robust standard errors clustered at the province level to take spatial correlation into account.[39] Descriptive statistics are provided in Table 4.1.

In order to estimate the effect of Protestantism on economic outcomes, we need an instrument to isolate exogenous variation in the share of Protestants in nineteenth-century Prussia. As argued in section 4.2, the possibility of reversed causality, omitted variables or both has already been raised by Weber (1904/1905) himself. We do not think that there is an instrument for Protestantism at the county level in our historical setting, which would fully satisfy the exclusion restriction. But we think that it is important to limit the influence of potential violations of the exclusion restriction as far as possible. Our preferred approach is to follow Spenkuch (2017) in using the idiosyncratic element of religious denomination as of 1624, the "normal year" for the Peace of Westphalia. All signatories of the peace treaty of 1648 agreed to accept the confessional situation as it prevailed in 1624, in particular not to force subjects to change their faith along with the faith of their ruler thereafter (Kampmann, 2008, pp. 176ff). This decision had a strong historical legacy (Schilling, 1994, pp. 99ff). The main idea of Spenkuch's instrument is to regress *Protestant in 1624* at the county level on predictors thereof, identified by Cantoni (2012) (latitude, contribution to Reichsmatrikel, distance to Wittenberg) and Rubin (2014) (existence of printing press) for counties in the former Holy Roman Empire (HRE). Distance to Wittenberg is also used for the construction of our IV, because distance to the powerful

[38] In order to differentiate between different Protestant denominations, we use census data from 1871 (Königlich Statistisches Bureau, 1875), which includes information on Protestant Reformist and other Protestants. Note, that less than 1 percent of the Prussian Population were Reformists.
[39] On the role of spatial correlation see Kelly (2019) and Colella *et al.* (2019).

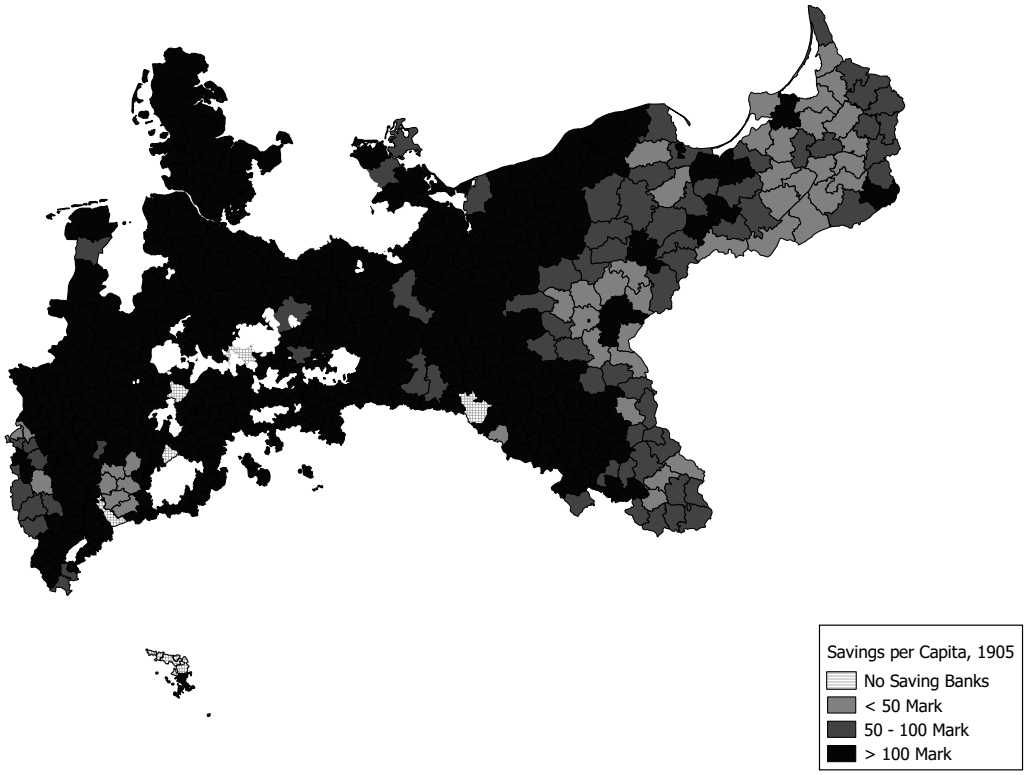
FIGURE 4.1
SAVINGS PER CAPITA, 1880



Notes: Darker colors correspond to higher savings per capita. White coloured counties do not have a savings bank in 1880.

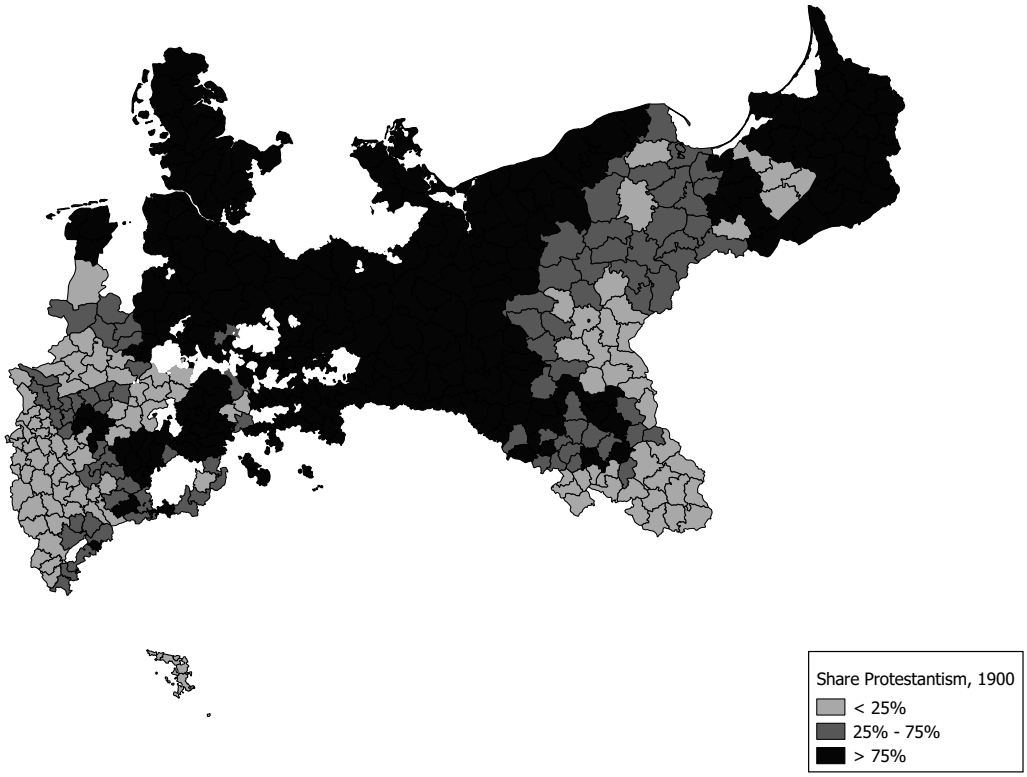
Source: See Table D1.

FIGURE 4.2
SAVINGS PER CAPITA, 1905



Notes: Darker colors correspond to higher savings per capita.
Source: See Table D1.

FIGURE 4.3
PROTESTANTISM, 1900



Notes: Darker colours correspond to a higher share of Protestants.
Source: See Table D1.

TABLE 4.1
DESCRIPTIVE STATISTICS

	1875	1880	1890	1900	1905
Savings					
Savings per Capita	42.34 (59.83)	64.77 (83.40)	94.63 (105.86)	153.03 (147.83)	206.15 (179.54)
Savings per Capita, including Polish credit cooperatives					206.96 (179.03)
Number Saving Banks	2.31 (2.59)	2.79 (2.89)	3.14 (3.16)	3.59 (3.70)	3.60 (3.15)
Religion					
Share Protestants	64.61 (37.71)	64.46 (37.59)	64.03 (37.31)	63.72 (36.90)	63.44 (36.57)
Share Other Religions	1.33 (1.30)	1.33 (1.23)	1.18 (1.09)	1.09 (1.00)	1.06 (0.97)
Nation					
Share German Speaking	87.83 (24.62)	87.83 (24.62)	87.83 (24.62)	87.90 (24.18)	87.90 (24.18)
Further Controls					
Income per Capita	313.55 (107.11)	313.55 (107.11)	421.54 (134.93)	529.53 (166.95)	583.52 (183.86)
Share Urban Population	26.96 (18.33)	27.86 (18.43)	29.23 (19.22)	31.11 (19.76)	32.12 (19.98)
Share Men Above 14	65.97 (3.61)	65.57 (3.24)	64.78 (2.99)	64.69 (3.53)	65.03 (3.67)
Average Household Size	4.74 (0.34)	4.79 (0.35)	4.73 (0.38)	4.70 (0.42)	4.70 (0.45)
N	434	434	434	434	434

Notes: Standard deviation in parentheses. Due to lack of data on employment statistics prior to 1882, we assume the same values for 1875 as for 1882. Our results do not change if we exclude 1875 from our analysis.
Sources: See Table D1.

state of Saxony may have played a role in strategic neighborhood interactions (Cantoni, 2012, pp.517-8). The residual of that regression is then used as instrumental variable.

The issue of reverse causation is addressed by using an instrument with a 200-year lag. By construction, the residual from this regression reflects factors that affected the probability of a county to be Protestant in 1624, but were orthogonal to any of these controls. For example, the residual could reflect a role for military campaigns, dynastic relations, marriage policies or historical accidents.[40] With this approach we directly address the problem of potential violations of the exclusion restriction, regarding the influence of persistent differences in economic development, or local characteristics such as ethnicity.

In particular, this approach has two main advantages in order to deal with ethnicity as confounding factor. First, the IV only includes counties that were part of the Holy Roman Empire, which reduces ethnic heterogeneity in our sample (but does not eliminate all of it).[41] Second, the control for distance to Wittenberg in combination with a control variable for latitude also helps to reduce the effect of ethnic heterogeneity, because differences in ethnicity have a clear geographical pattern (compare Figures 4.3 and 4.4). Overall, the correlation between our IV and the share of German-speaking population is low (0.07) and insignificant.

The most prominent alternative to Spenkuch (2017) is the idea of Becker and Woessmann (2009) to use distance to Wittenberg directly as an IV for religious denomination. Such an approach, just like any other distance-based instrument is likely to violate the exclusion restriction, given the geography of differences in ethnicity.[42] For instance, the correlation between distance to Wittenberg and share of German-speaking population is quite high (–0.35) and significant (both for the full sample and a sample limited to the HRE). The correlation is actually similar in size to the correlation between distance to Wittenberg and the share of Protestants (–0.37).

As with any IV, we cannot rule out that our residual approach still suffers from violations of the exclusion restriction. But we have formally tested whether a violation of the exclusion restriction would affect our results, following Conley *et al.* (2012). The test indicates that our approach is quite robust, while the alternative IV would be very sensitive to a violation of the exclusion restriction.[43]

[40] Since the reformation, religion was a major factor for dynastic marriage policy, see for example Duchhardt (2001) or Schönpflug (2013).
[41] Hence, with our instrument we exclude several counties in the eastern districts Bromberg, Danzig, Gumbinnen, Königsberg, Köslin, Marienwerder, Posen as well as some counties in the northern district Schleswig-Holstein.
[42] Of the 50 counties furthest from Wittenberg 44 are in the eastern provinces and only 6 are in the southwest of Prussia.
[43] Following Karadja and Prawitz (2019), if we allow a direct negative standardized effect of distance to Wittenberg on literacy of -0.05, the coefficient for Protestantism becomes insignificant. Note that the standardized reduced form effect of distance to Wittenberg is around -0.37. Thus, if we allow for only slight violations of the exclusion restriction, the result already turns insignificant. If we rely on our preferred instrument, this is not the case. Here, we can allow for a direct effect of more than half of the standardized reduced form effect and still find significant effects.

Based on our discussion, we estimate the following two-stage least squares model:

$$\begin{aligned} Prot_{it} &= \alpha_2 + \beta_2 ResidualProtestant1624_i + X'_{it}\gamma_2 + \epsilon_{2it} \\ Sav_{it} &= \alpha_3 + \beta_3 \widehat{Prot_{it}} + X'_{it}\gamma_3 + \epsilon_{3it} \end{aligned} \tag{4.2}$$

In the next step, we test for the effect of Protestantism on literacy rates, as suggested by Becker and Woessmann (2009). They argued that Weber was mistaken to suggest that Protestant attitudes mattered, but that instead Protestantism helped spread the Bible and the ability to read (and write). This way Protestantism may have had an effect on literacy and thereby on economic development. In contrast to our data for savings, literacy rates are only available for one cross-section in 1871, hence we run the following specification

$$Lit_i = \alpha_4 + \beta_4 Prot_i + X'_i\gamma_4 + \epsilon_{4i} \tag{4.3}$$

where Lit_i is the share of literates in a county's population aged 10 or older, $Prot_i$ is the share of Protestants in the county and X_i is the same set of demographic control variables as used in Becker and Woessmann (2009).[44]

$$\begin{aligned} Prot_i &= \alpha_5 + \beta_5 ResidualProtestant1624_i + X'_i\gamma_5 + \epsilon_{5i} \\ Lit_i &= \alpha_6 + \beta_6 \widehat{Prot_i} + X'_i\gamma_6 + \epsilon_{6i} \end{aligned} \tag{4.4}$$

What is more important, given the potential pitfalls of OLS and IV regressions in our context, we provide direct statistical evidence on the difference in literacy rates between Catholics and Protestants within counties. We use a cross table on literacy by religion, which was provided by the Königlich Statistisches Bureau (1875), digitalized by Galloway (2007), but largely neglected in the literature so far.[45]

Third, we test for the role of ethnic differences. We include the share of people whose mother tongue is German as an indicator for ethnic differences. Figure 4 shows the geographic distribution of the share of people with German as their mother tongue. Minorities were mainly concentrated in the eastern provinces as well as in the northern part close to the Danish border. In addition, there is also a smaller Polish minority in the Ruhr area, the so-called *Ruhrpolen* (Ruhr Poles). Note, that this variable is available for the full sample only from 1890 onwards. In 1890, German was the mother tongue of 87.8 percent of the population in Prussia, Polish for 9.6 percent and other languages (e.g. Danish) for 2.6 percent. We will use this information for all years and assume that it does not vary over time.[46] Comparing Figures 3 and 4 show

[44] Note that there is no indication that the census was biased towards German speaking. The question in the census asked whether people aged 10 or older were able to read and speak. It did not specifically ask about the ability to speak German (Bureau des Ministeriums des Innern, 1871).
[45] Unfortunately, data on literacy by ethnic nationality is not available.
[46] For a large sub-sample, data is available for 1867. Comparing the data from 1867 and 1890 shows no major deviations. Thus, it seems plausible to use the data from 1890 for 1875 and the following years.

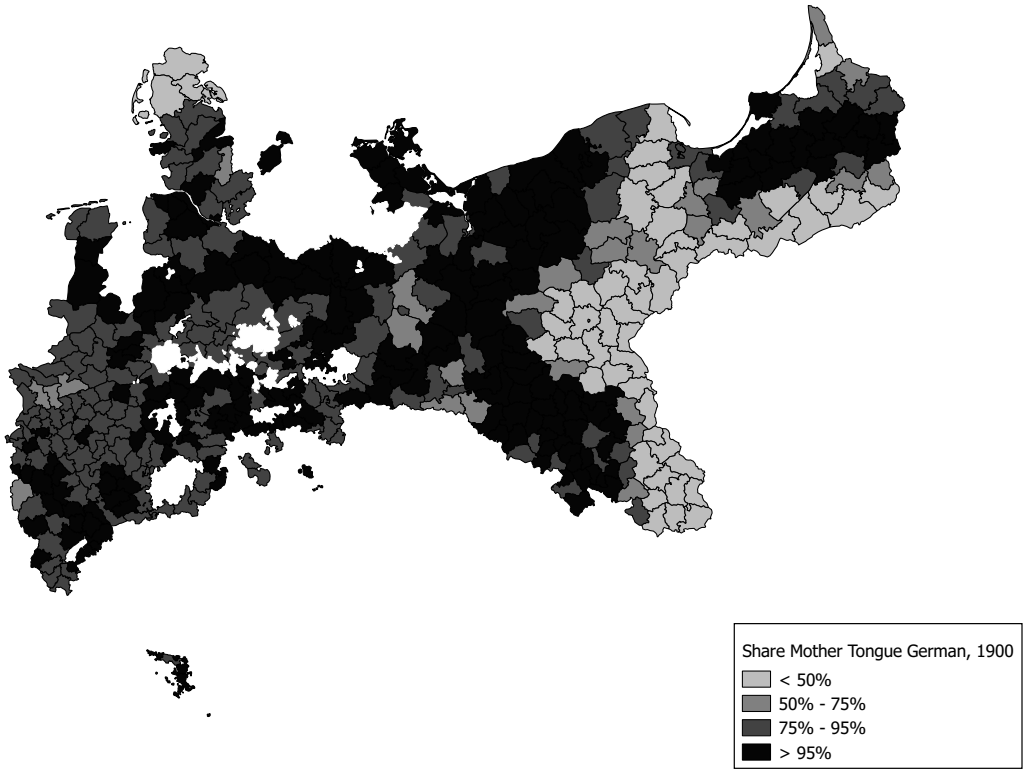
some correlation between religious denomination and ethnic group, especially in the eastern part of Prussia. However, there is considerable variation in terms of religious denomination in the ethnically homogeneous western parts of Prussia, for example, in the Rhine Province.

Similar to religion, ethnic differences might also be endogenous. In order to circumvent this problem, we will show our results including and excluding the eastern provinces East and West Prussia, Poznan, and Silesia. This also has the advantage that we avoid possible multicollinearity between religious denomination and ethnic groups in the eastern regions, which makes it difficult to disentangle the effect between the two variables. The remaining parts of Germany show very large variation in terms of religious denomination with quite small ethnic minorities, which should be sufficient to identify an effect of Protestantism on economic outcomes.

A specific problem would arise with regard to our data from savings banks, if the Polish minority would consider these banks as German institutions and prefer to deposit their savings elsewhere. There is no evidence for institutional barriers for the Polish minority to get access to the savings banks (Trende, 1957, p.93). But there has been a contemporary discussion on this and evidence that Polish credit cooperatives competed for Polish savings with growing success, especially after the turn of the century (Bernhard 1907, p.244, see also Guinnane 2001). This could obviously bias our results towards finding an effect of ethnic differences on saving, because we would underestimate savings from the Polish community. To deal with this we collected data on the deposits of Polish credit cooperatives at the county level as of 1905 for the two provinces in Prussia with the highest share of Polish speakers (and correspondingly lowest share of German speakers), Poznan and West Prussia. The data comes from the balance sheets of all 225 credit cooperatives that were members of the "Association of Polish Economic Cooperatives in the Provinces of Poznan and West Prussia", and were published by Prussian authorities (see Suesse and Wolf 2020, Appendix G).[47] Given that the vast majority of Polish savings were deposited at Polish credit cooperatives as argued by Bernhard (1907, p.404), the addition of savings deposits at Polish credit cooperatives to savings deposits at savings banks should correct for such a potential bias. Note that including saving deposits at Polish credit cooperatives matters for total savings in West Prussia and Poznan, for example, for Poznan this addition leads to an increase in total savings per capita from 73 RM to 84 RM.

[47] Similar data for East Prusia and Silesia, which also had high shares of Polish speakers do not exist, but we can show that our results are robust to their exclusion (Table 4.4, Cols. 3 and 6).

FIGURE 4.4
MOTHER TONGUE GERMAN, 1890



Notes: Darker colors correspond a higher share of German speakers in the population.
Source: See Table D1.

4.5 Results

Testing the "Common Interpretation": OLS

Let us start with some simple correlations between the share of Protestants and the share of Germans with income, savings, and literacy rates.[48] For this, we run plain OLS regressions with labour income per capita (1875-1905), savings per capita (1875-1905), income per capita measured by income tax statistics (1871) as well as literacy rates (1871) as outcome variables. Table 4.2 provides some first suggestive evidence on our three hypotheses. Protestantism does not seem to be strongly correlated with savings per capita nor with labour income per capita. There is a correlation between Protestantism and literacy rates, but a much stronger one between the share of German speakers and literacy rates (see Table 4.2 Panel 4, Column 2). And, more generally, we find that the standardized coefficient on the share of German speakers is larger for all outcomes than the one on the share of Protestants. But correlation is not causation, so we need to turn to an IV analysis.

Causal effects on savings

To get as close as possible to the thrust of Weber's ideas, we first test for savings behavior as a mechanism for the "common interpretation". Table 4.3 provides details on the relationship between savings per capita and religion from pooled OLS and two-stage least squares (2SLS) regressions. The results do not support the hypothesis according to which Protestants have a higher savings per capita than Catholics due to a specific work ethic and ascetic lifestyle. This "non-result" holds independent of whether we include counties in eastern districts or not (Columns 1 and 2) and whether we control for labour income per capita (Columns 3 and 4). As expected, labour income per capita is positively correlated with savings per capita. In additional regressions (reported in the Appendix in Table D2) we allow for heterogeneous effects for reformed and other Protestants. We do not find support for the idea that reformed Protestants have higher savings per capita or higher saving rates. If anything, reformed Protestants seem to have lower savings per capita. We conclude that we have to reject hypothesis H1, based on our evidence for Prussia after 1871. At the time of Weber's writing, there is no evidence that Protestants would have saved more than Catholics.

Our reading of Weber's PE suggests that we should not be surprised by this. Given the context of rising nationalism and ethnic tensions especially between Germans and Poles, we might wonder instead if there were substantial differences in saving behavior between ethnic groups. A problem here is that our data from savings banks might systematically underestimate Polish savings, if Poles indeed started to prefer Polish credit cooperatives for their saving deposits as argued

[48] The replication files are provided in Kersting *et al.* (2020).

TABLE 4.2
GERMAN SHARE, PROTESTANTISM AND ECONOMIC OUTCOMES

	(1)	(2)
Panel 1: Savings per Capita, 1875-1905		
Share Protestants	0.042	0.021
	(0.075)	(0.085)
Share German Speaking		0.095
		(0.054)
Further Controls	Y	Y
N	2170	2170
R^2	0.287	0.293
Panel 2: Labor Income per Capita, 1875-1905		
Share Protestants	-0.054	-0.077
	(0.081)	(0.077)
Share German Speaking		0.112*
		(0.059)
Further Controls	Y	Y
N	2170	2170
R^2	0.370	0.379
Panel 3: Income Tax Revenue per Capita, 1871		
Share Protestants	0.170*	0.129
	(0.091)	(0.095)
Share German Speaking		0.244
		(0.160)
Further Controls	Y	Y
N	426	426
R^2	0.332	0.336
Panel 4: Literacy, 1871		
Share Protestants	0.099***	0.062***
	(0.010)	(0.008)
Share German Speaking		0.221***
		(0.014)
Further Controls	Y	Y
N	452	452
R^2	0.737	0.831

Notes: In Panel 1 and 2 standardized beta coefficients. Standard errors in parentheses. Robust standard errors clustered at the province level. Further controls include in Panel 1 and 2: number of savings banks, share working men above 14, share urban population, average household size, share other religions, dummy for counties w/o savings bank. Further controls include in Panel 3 and 4: % age below 10, % Jews, % females, % born in municipality, % of Prussian origin, average household size, population size (log), population growth 1867-1871 (in %), % missing education info, % blind, % deaf-mute, % insane. * p<0.1, ** p<0.05, *** p<0.01.
Sources: See Table D1.

TABLE 4.3
SAVINGS PER CAPITA, 1875-1905

	(1)	(2)	(3)	(4)
Panel 1: OLS	Dep. Var. Savings per Capita			
Share Protestants	0.012	0.022	0.033	0.065
	(0.079)	(0.102)	(0.081)	(0.103)
Income per Capita			0.350***	0.316***
			(0.068)	(0.072)
R^2	0.280	0.245	0.351	0.308
Panel 2: Second Stage	Dep. Var. Savings per Capita			
Share Protestants	-0.034	-0.029	-0.057	-0.064
	(0.101)	(0.183)	(0.110)	(0.193)
Income per Capita			0.342***	0.297***
			(0.072)	(0.084)
Panel 3: First Stage	Dep. Var. Protestantism			
Residual Decision 1624	0.429***	0.288***	0.432***	0.294***
	(0.083)	(0.048)	(0.081)	(0.045)
Income per Capita			-0.109	-0.162*
			(0.092)	(0.080)
R^2	0.324	0.392	0.331	0.409
Including Eastern Provinces	Y	N	Y	N
Further Controls	Y	Y	Y	Y
F-Stat excluded instrument	26.65	36.26	28.73	42.32
N	1830	1355	1830	1355

Notes: Standardized beta coefficients. Standard errors in parentheses. Robust standard errors clustered at the province level. Eastern provinces include East and West Prussia, Poznan, and Silesia. Further Controls include number of savings banks, share working men above 14, share urban population, average household size, share other religions, dummy for counties without a savings bank. * p<0.1, ** p<0.05, *** p<0.01.
Sources: See Table D1.

above. However, this effect is unlikely to drive our results. First, it noteworthy that we do not find any significant relationship between savings and Protestantism in Table 4.3 (Column 1), even though there might be a bias against savings by Poles, which were predominantly Catholic. Next, we can directly correct for such a bias with data on saving deposits from Polish credit cooperatives, which we could find for 1905. In Table 4.4, we rerun our regression from Table 4.3 with a control variable for the share of German speakers (Columns 1 and 2). We see that ethnic differences are strongly correlated with saving behavior, while religious differences are not. If we add saving deposits from Polish credit cooperatives to our dependent variable this result remains virtually unchanged (columns 4-6 in Table 4.4). Given that we have the data for Polish savings only for counties in the Provinces of Poznan and West Prussia, we show that our main findings remain qualitatively unchanged, if we drop the other two provinces with large Polish populations, Silesia and East Prussia (Table 4.3, Col. 3 compared to Col. 6).[49] For each specification, the coefficient on German speakers declines very slightly but stays large and significant.[50] It seems unlikely that this effect is due to a Polish bias against savings banks. In any case, our results suggest that ethnic differences were strongly correlated with economic outcomes (H3).

Causal effects on literacy rates

While there is no evidence for late nineteenth-century Prussia, that Protestants had higher savings per capita, it could still be that Protestantism mattered for economic development via some other mechanism, such as literacy rates as argued by Becker and Woessmann (2009).

As a first step to test hypothesis H2, we analyze a cross-table, which includes information on literacy by religion as of 1871. This evidence is essential, as it directly shows how literacy rates differed between Protestants and Catholics within counties. It is quite surprising that this has been largely ignored in the existing empirical literature. In Figure 5 we plot the share of Protestants and the share of Protestant literates in literates. Each dot represents one county. The intuition is the following: a dot right to the 45-degree line indicates that Protestants are over-proportionally literate in this county. Panel (a) in Figure 4.5 shows that there is some evidence that Protestants indeed are over-proportionally literate. However, most counties in which Protestants are over-proportionally literate are located in the eastern part of Prussia with a large Polish-speaking population as seen in Panel (b) in Figure 4.5. In counties excluding the eastern provinces, the relationship between Protestant literates and Protestants follows

[49] The shares of German speakers in the population (1890) were 38.88 percent (Poznan), 61.87 percent (West Prussia), 76.38 percent (East Prussia), 77.06 percent (Silesia), 81.22 percent in Schleswig-Holstein and around 98 percent in all remaining provinces of Prussia.

[50] Given that our instrument excludes some counties in the eastern provinces, we run further OLS regressions in Appendix Table D3. Here we include all Prussian counties, except the provinces of Silesia and East Prussia where we do not have additional data on Polish savings. As in Table 4.4 before, we find a strong positive correlation between the share of German-speaking population and the savings per capita. The coefficient of share of German-speaking population declines slightly but stays highly significant, once we control for Polish savings.

TABLE 4.4
SAVINGS PER CAPITA AND ETHNIC DIFFERENCES, 1905

	(1)	(2)	(3)	(4)	(5)	(6)
Panel 1: OLS	Dep. Var. Savings p.c.			+ Pol. credit coop.		
Share Protestants	0.067	0.069	0.081	0.067	0.069	0.081
	(0.085)	(0.085)	(0.116)	(0.085)	(0.085)	(0.116)
Share German Speaking	0.121**	0.116**	0.113***	0.121**	0.116**	0.112***
	(0.041)	(0.049)	(0.027)	(0.041)	(0.049)	(0.027)
Income per Capita		0.033	-0.051		0.032	-0.052
		(0.092)	(0.078)		(0.092)	(0.078)
R^2	0.291	0.292	0.252	0.291	0.292	0.251
Panel 2: Second Stage	Dep. Var. Savings p.c.			+ Pol. credit coop.		
Share Protestants	-0.011	-0.013	-0.069	-0.012	-0.014	-0.070
	(0.107)	(0.103)	(0.181)	(0.107)	(0.104)	(0.181)
Share German Speaking	0.129***	0.126**	0.129***	0.128***	0.125**	0.128***
	(0.041)	(0.050)	(0.035)	(0.040)	(0.050)	(0.035)
Income per Capita		0.023	-0.080		0.023	-0.080
		(0.095)	(0.096)		(0.095)	(0.096)
Panel 3: First Stage	Dep. Var. Protestantism					
Residual Decision 1624	0.407***	0.414***	0.262***	0.407***	0.414***	0.262***
	(0.096)	(0.090)	(0.063)	(0.096)	(0.090)	(0.063)
Share German Speaking	0.065	0.087	0.063	0.065	0.087	0.063
	(0.104)	(0.094)	(0.067)	(0.104)	(0.094)	(0.067)
Income per Capita		-0.148	-0.223		-0.148	-0.223
		(0.159)	(0.138)		(0.159)	(0.138)
R^2	0.331	0.343	0.424	0.331	0.343	0.424
Silesia and East Prussia	Y	Y	N	Y	Y	N
Further Controls	Y	Y	Y	Y	Y	Y
F-Stat excluded instrument	17.85	21.01	17.20	17.85	21.01	17.20
N	366	366	281	366	366	281

Notes: Standardized beta coefficients. Standard errors in parentheses. Robust standard errors clustered at the province level. Further Controls include number of savings banks, share working men above 14, share urban population, average household size, share other religions, dummy for counties without a savings bank. * p<0.1, ** p<0.05, *** p<0.01. In columns (1) to (3) we include savings per capita from saving-banks, in columns (4) to (6) we add deposits from Polish credit cooperatives.
Sources: See Table D1.

very closely a 45-degree line as shown in Panel (c) in Figure 4.5. The difference in literacy rates between Catholics and Protestants within counties is a mere 1.05 percentage points for the entire sample. Once we exclude the eastern provinces, this difference declines to 0.14 percentage points. Another way to illustrate this is to show the difference in the share of Protestant literates in literates and share of Protestants on a map: Figure 4.6 shows that the counties with over-proportional literacy of Protestants are exclusively located in the eastern part of Prussia.[51]

Table 4.5 shows the results from an OLS and IV approach to the problem. To compare to our results on savings and to the previous literature, we focus again on *average literacy rates* at the county level. To start with, the OLS results suggest some small but significant effect of Protestantism on average county-level literacy (Column 1). They are still smaller if we exclude the Eastern provinces (Col. 2), or if we include them but control for the share of German speakers (Col. 3), and much smaller than those for German speakers. However, the OLS coefficient stays statistically significant. A possible reason for this could be a county-specific Protestant tradition of education, which could have had positive spillovers to Catholics, along the lines suggested by Becker and Woessmann (2009).[52]

Alternatively, the OLS result could reflect reverse causation running from literacy to Protestantism or a problem of omitted variables at the county-level as discussed in section 4.4. To address this, we need an instrument and use again the IV from Spenkuch (2017). Our findings are very much in line with the evidence from the cross-table visualized in Figures 4.5 and 4.6.We find no significant positive effect of Protestantism on literacy rates (Table 4.5, Panel 2, Columns 1 to 3). Moreover, we have tested for the idea that Protestantism affects economic outcomes with literacy as a mediating variable but do not find much support for this (see Appendix D.3).[53] In Table 4.5 (Column 3), we see that the effect of the share of German speakers is large and highly significant. An increase in the share of the population with German as their mother tongue by 1 percentage is associated with an increase in literacy by 0.2 percentage points. Given the size of this effect, ethnic differences clearly should be a control variable for the "common interpretation", notably for nineteenth-century Germany. Overall, our results show strong support for H3, but neither for H1 nor H2.

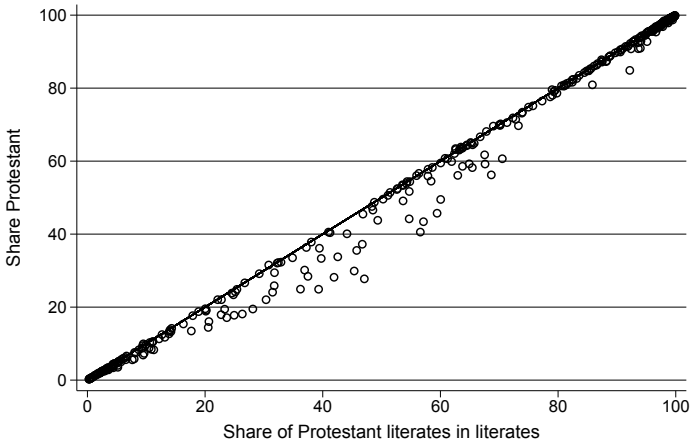
[51] There are only a few counties in the rest of Prussia, where this difference amounts to more than 1 percentage points. This is the case for the counties Moers, Rees, Gladbach, and Wiedenbrück. Note that even this difference is insignificant.

[52] In Table D7 we exploit the cross-table data to test spill-over effects of Protestantism in Becker and Woessmann (2009). To this end, we use literacy among Catholics at the county-level as our dependent variable and test, whether the share of Protestants in a county has a positive effect. The OLS regression (Panel 1) does not suggest any effect. An IV regression with distance to Wittenberg suggests a weakly significant positive effect, but only if we restrict the sample to the HRE. Using the residual of 1624 as an instrument, we do not find any effect of the share of Protestants on literacy rates among Catholics.

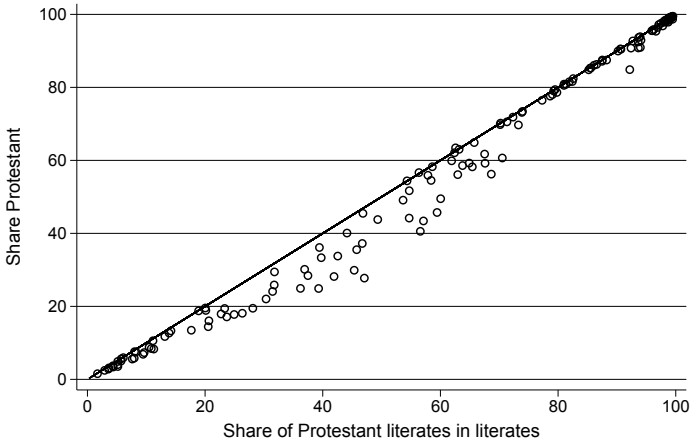
[53] In Appendix D.3 we compare our results more directly with the one from Becker and Woessmann (2009). There we replicate their findings and show how controlling for German speakers, changes in the sampe and the use of distance to Wittenberg as IV lead to different results.

FIGURE 4.5
LITERACY AND PROTESTANTISM, 1871

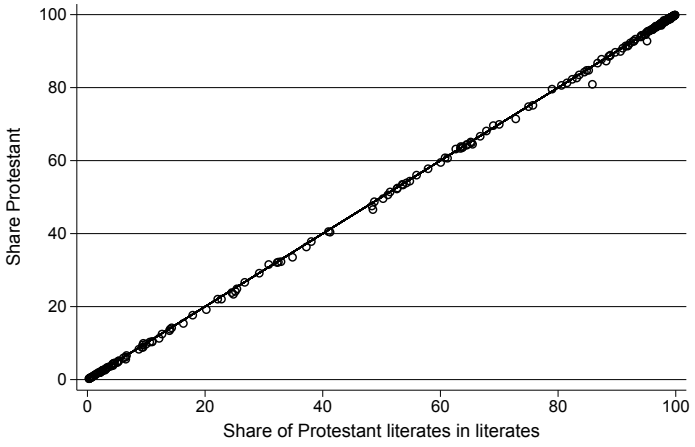
(A) ALL COUNTIES



(B) EASTERN PROVINCES



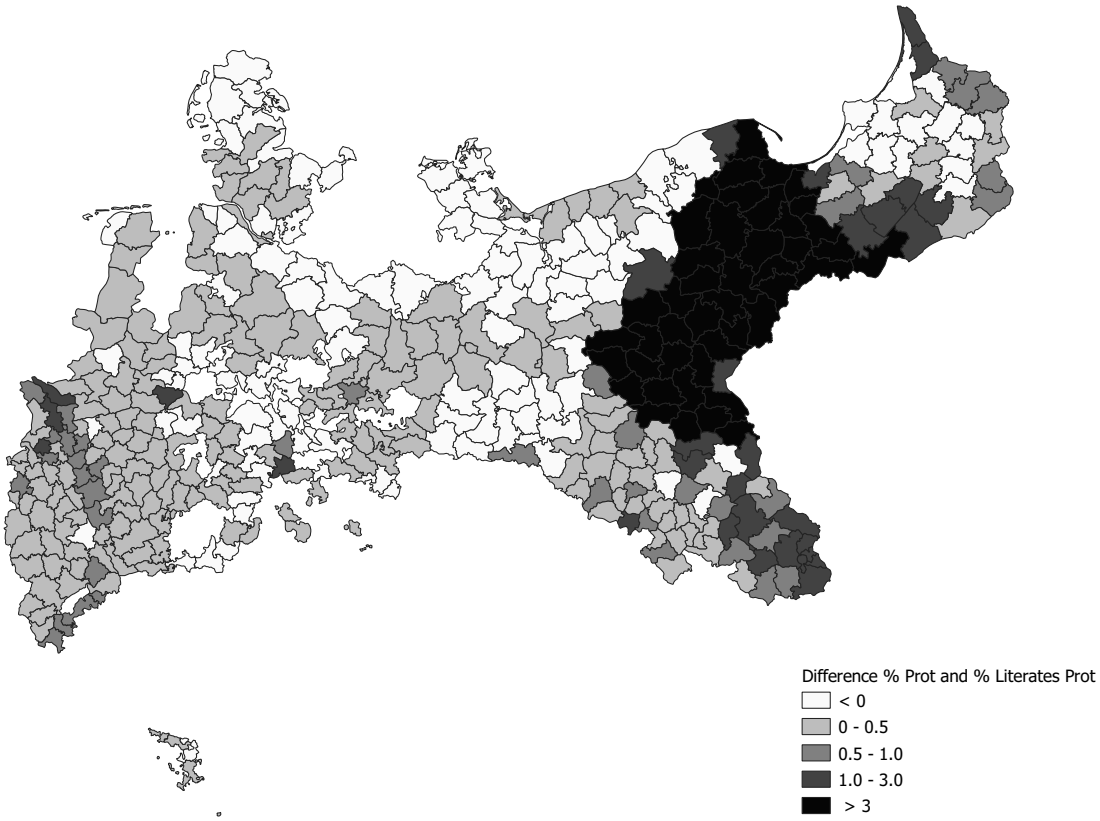
(C) WESTERN PROVINCES



Notes: Each dot corresponds to one county. Interpretation: protestants are over-proportionally literate in counties to the right of the 45 degree line. Eastern Provinces: Poznan, Silesia, West and East Prussia.

Source: See Table D1.

FIGURE 4.6
DIFFERENCE % PROTESTANT LITERATES IN LITERATES AND % PROTESTANTS, 1871



Notes: The maps shows for each county the difference between the share of Protestants and the share of Protestant literates in literates. The stronger Protestants are over-proportionately literate, the darker the color for this county.

Source: See Table D1.

TABLE 4.5
EFFECT OF PROTESTANTISM ON LITERACY, 1871

	(1)	(2)	(3)
Panel 1: OLS	Dep. Var. Literacy		
Share Protestants	0.057***	0.033**	0.040***
	(0.009)	(0.013)	(0.012)
Share German Speaking			0.213***
			(0.036)
R^2	0.610	0.431	0.735
Panel 2: Second Stage	Dep. Var. Literacy		
Share Protestants	0.030	0.031	0.020
	(0.018)	(0.017)	(0.015)
Share German Speaking			0.221***
			(0.036)
Panel 3: First Stage	Dep. Var. Protestantism		
Residual Decision 1624	46.088***	43.094***	45.369***
	(2.936)	(3.277)	(2.866)
Share German Speaking			0.327**
			(0.144)
R^2	0.474	0.599	0.489
Including Eastern Provinces	Y	N	Y
Further controls	Y	Y	Y
F-Stat excluded instrument	246.34	172.95	250.58
N	378	280	378

Notes: Standard errors in parentheses. Robust standard errors clustered at the province level. Eastern provinces include East and West Prussia, Poznan, and Silesia. Further controls: % age below 10, % Jews, % females, % born in municipality, % of Prussian origin, average household size, population size (log), population growth 1867-1871 (in %), % missing education info, % blind, % deaf-mute, % insane. * p<0.1, ** p<0.05, *** p<0.01.
Sources: See Table D1.

4.6 Why do ethnic differences matter?

Our finding on the role of ethnic differences for economic outcomes begs for some explanation. We think it unlikely that ethnicity captures any deep differences in attitudes between Germans, Poles and other ethnic groups, as argued by Weber in his IL 1895. Instead, we suggest that the effects might result from the interaction between Germans and Poles. In particular, we provide some suggestive evidence on the role of discrimination against the Polish minority by the German majority and on the Polish institution building as a reaction against this discrimination. It would be beyond the scope of our paper to analyze this in detail, but we think it should be taken up by further research.

As described in Section 4.3 on the historical context of our study, tensions between nationalities increased within the German Empire after its foundation. One – or rather the major aspect of these tensions was discrimination against the large Polish minority concerning education policy. The Prussian government had pursued discriminatory policies against the Poles already before 1870, especially restrictive language policies after 1848 (Gessinger, 1991, pp.118ff). After 1870 the German government provided monetary incentives (so-called *Ostmarkenzulage*) for German teachers to work in the eastern provinces, which attracted especially more nationalist teachers with a mission in "Germanizing" the East (Lamberti, 1989). Anecdotal evidence suggests that this had striking effects. For example, in Poznan, the largest Polish city in the Empire, the most prestigious Catholic grammar school (St Mary Magdalene School) had 24 Polish teachers in 1870, but only 3 in 1890 and 2 in 1912 (Molik, 1998, p.22). Another result of this discriminatory policy was that the expenses per pupil were lower in the eastern provinces (Lamberti, 1989). Regions in Prussia with a higher linguistic polarization, that is, especially the eastern provinces, tended to spend less for the decentralized education system (Cinnirella and Schueler, 2016). Moreover, there is evidence that schools in the eastern provinces with a higher share of German-speaking pupils were preferred in terms of resources (Lamberti, 1989). Another important aspect of this is higher education. In spite of decades of discussion around the subject, which started already in 1815 and intensified again after 1871, there was no university in these provinces, in contrast to the Austrian partition of Poland (with the universities of Krakow and Lwow, founded 1364 and 1661, respectively), and the Russian partition (with the university Warsaw, opened in 1815, closed in 1869). Polish speakers had to attend a university in one of the German provinces and study in German. According to Schutte (2008), the German reluctance to open a university in the Polish provinces was due to the fear of the German majority that better education might equip the Polish minority with more effective tools for resistance against discrimination and own nation-building. The 1903 opening of a Royal Academy in Poznan that served some limited functions of a university (in German language) was accompanied by a heated debate. It was only in 1919 after Poland was re-established as a state, that the Academy was turned into a full university, which could cater the needs of the Polish society.

This discrimination in terms of education policy had probably direct effects on educational attainment and human capital formation. Insofar as this discrimination started already before

1871, for example in the realm of language policy and higher education, it might help to explain the differences in literacy rates around 1871 (see the discussion in Knabe 2000, pp.162ff. Such discrimination would also have effects on earnings and incomes, but not necessarily on savings rates unless it affected savings stronger than incomes. As suggested by the literature on financial literacy, this is indeed likely to be the case (Lusardi and Mitchell, 2011), but it would require a separate investigation.

Additionally, the existence of increasing national segregation in the labor market might help to explain the difference in economic outcomes. The occupational distribution can be used as an indicator for the status of different groups in a society as shown by Hsieh *et al.* (2013) for the United States. These authors also show that frictions in the labour markets, which can be due to discrimination, can lead to a systematic misallocation of talent with far-reaching implications for economic development. According to Hagen (1981), there were striking differences in the occupational structure between Germans and Poles. The ratio between Germans and Poles for different occupations in the province Poznan was for medical doctors 3 to 1, veterinarians 8 to 1, apothecaries 3 to 1, lawyers and notaries 4 to 1 and employees in railway and postal offices 19 to 1. Given that the share of Polish people in Poznan was about 50 percent, these statistics show a very clear national divide in the labor market. The contemporary statistician Max Broesike (1909) provides further evidence for a sharp segregation between Poles and Germans in the labor market in the province Silesia: Poles were underrepresented in the industry sector and especially in public services. Instead, Poles were concentrated in the agricultural sector. Already from the 1860s onward were the higher ranks in the public administration of Ostelbien nearly exclusively dominated by Germans (Molik, 1998). Among all academic professions, medical doctors were the most attractive for the Polish minority, also due to the relative independence from state intervention. The number of Polish doctors in the province of Poznan increased between 1872 and 1912 in absolute terms but also relative to the number of German doctors (Molik, 1998). But the differences remained striking. Table D4 in the Appendix provides detailed evidence on doctors for each district in the province Poznan for 1907 and shows sharp differences between the share of Polish doctors and the share of Polish-speaking population. To summarize, there is some evidence suggesting that anti-Polish discrimination is a potential explanation for the large differences in economic outcomes between Germans and Poles. We cannot substantiate these ideas here but think they could be fruitful avenues for future research.

4.7 Conclusion

We revisited the evidence for Max Weber's PE in the context in which he was writing: The German Empire before 1914. To speak with Robert Margo, this is an example why "putting the context front and center is the essence of economic history, its fundamental contribution to economics per se" (Margo, 2017, p.37). We showed that a misinterpretation of this context can easily lead to missing the main factors in the evidence, including mistaken econometric specifications. Our main argument is that the "common interpretation" of Weber's PE has

largely missed his own focus on saving behavior and his anti-Polish nationalism. First, we test Weber's suggestion that Protestantism mattered due to an "ascetic compulsion to save". Using data for late nineteenth-century Prussia we can clearly reject this hypothesis. Neither in simple OLS nor in IV regressions, nor for subsamples do we find that Protestants saved more than Catholics (Tables 4.3 and 4.4). However, there is evidence that saving behavior differed between Germans and Poles (Table 4.4). Next, we test the hypothesis that Protestantism mattered via differences in literacy rates. We show from direct statistical evidence that such differences were negligible for the predominantly German-speaking provinces of Prussia and mattered only in the East, in regions with a large Polish minority (Figure 4.5). We confirm these findings using OLS and IV regressions (Table 4.5). Taken together, we show that economic outcomes in late nineteenth-century Prussia differed much more between ethnic groups than between religious groups.

This new empirical evidence is in line with the bulk of the historical literature, which stresses the increasing role of national conflict between Germans and national minorities towards the end of the nineteenth-century, while tensions between Protestants and Catholics were abating after the end of the Kulturkampf. The German authorities used their power, in the realm of education policy and elsewhere, which were increasingly geared against the Polish minority. We provided some tentative evidence that Poles suffered from discrimination in education policies and in the labour markets. We also showed that the successful emergence of Polish parallel structures, such as Polish credit cooperatives used as substitutes for savings banks, are relevant in our context. On another level, we find that this is in line with a critical reading of Weber's PE, which should be understood in its historical context. Weber himself was an ardent German nationalist, and it would be naive to consider the PE only as an attempt to explain the origins of capitalism. It is certainly a founding text for sociology and cultural economics. But beyond this, it should be understood as a political intervention that aimed to provide the German political class with a "calling". On a final note, we do not want to dismiss a more abstract interpretation of Weber's writing from the perspective of empirical economics. This can be stimulating and generate valuable insights. But our evidence cautions that the "common interpretation" of Weber's PE should take nationalism and ethnic differences into consideration, in the context of nineteenth-century Germany or elsewhere.

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Appendices

Appendix A

'On the origins of national identity. German nation-building after Napoleon' (Chapter 1)

A.1 Matching description

For our statistical analysis to be feasible, we first have to determine which children in our records belong to the same family. Our goal is to create a common family ID for all children that have the same mother and father. As is often the case with historical records, these are difficult to match as they frequently contain spelling mistakes. Therefore, we cannot use exact name matches but instead measure similarity between strings with the so called Jaro-Winkler (JW) distance (Winkler, 1990).

Abramitzky *et al.* (2019a) suggest some best practices for linking historical data. They are primarily concerned with how to match census data. They suggest a more conservative threshold of a JW distance less than 0.1 for two names to be considered a match. Additionally, they restrict their potential matches to people with the same place of birth and also apply a maximum age difference. Fouka (2020) uses only JW distances to match census records.

There are several properties of our dataset that make it less probable from the beginning to get false positive matches which means that we can be a bit less conservative than suggested by Abramitzky *et al.* (2019a). First of all, we only consider potential matches in the same city. Second, our dataset comprises only a relatively short time window, from 1810 to 1821. Third, we use the mother's and father's last name for our matching approach. First names are more unreliable as their number and order is often different for the same person across different entries in our historical data. As Feigenbaum (2016) notes, last names of matched entries in the IPUMS database usually have less of a JW distance than first names. He finds that over 99% of

all linked entries have a JW distance of less than 0.2. Last names also have a higher variation than first names which means that there is a relatively small probability of a married couple having exactly the same two last names as another couple. Additionally, we have the advantage that we can use two last names instead of just one in the case of census linking.

Considering the approaches cited above, we choose a JW distance threshold of 0.2. Our matching approach then goes as follows:

- Restrict the dataset to one city at a time. This means that we only consider potential matches in the same city.
- Split the data according to last name initials of mothers and fathers, similar to the blocking approach used by Abramitzky *et al.* (2019b).
- In these subsets, we calculate the string distances between mothers' and fathers' last names separately, using last names before 1815 as the rows of our distance matrix and last names starting in 1815 as the columns. We find all instances where both the mother's last name and the father's last name from one couple match with the last names of another couple.
- We group these instances together as a family. For example, if couple a before 1815 matches couples x, y and z after 1815 and couple b before 1815 also matches x, y and z, we group all five together as the same family.

A.2 Tables and figures

Table A1
Treatment Analysis 1815, Event Study

Dep. var.: National First Name	City FE (1)
Treated × 1810	0.006
	(0.016)
Treated × 1811	0.009
	(0.016)
Treated × 1812	0.021
	(0.017)
Treated × 1813	-0.007
	(0.016)
Treated × 1815	0.020
	(0.017)
Treated × 1816	0.046**
	(0.018)
Treated × 1817	0.023
	(0.018)
Treated × 1818	0.032*
	(0.018)
Treated × 1819	0.033*
	(0.017)
Treated × 1820	0.019
	(0.017)
Treated × 1821	0.039**
	(0.017)
Year FE	✓
City FE	✓
Observations	60972
Families	38766
R-squared	0.03

Notes: * p<0.1, ** p<0.05, *** p<0.01. We only include parents without national first given name. Clustered standard errors at the family level. Treated cities: Aachen and Muenster. Control cities: Frankfurt (Main), Hanover, Heidelberg, Nuremberg, and Mannheim. Results based on equation 1.2. Reference year: 1814.

TABLE A2
TREATMENT ANALYSIS 1815, ROBUST

Dep. var.: National First Name	City FE		Family FE	
	(1)	(2)	(3)	(4)
Panel 1: Control group Berlin				
Treated × Post1815	0.046***		0.095**	
	(0.010)		(0.045)	
Post1815	-0.009	-0.009	0.103***	0.103***
	(0.007)	(0.007)	(0.028)	(0.028)
Aachen × Post1815		0.033***		0.052
		(0.012)		(0.052)
Muenster × Post1815		0.071***		0.185***
		(0.014)		(0.068)
City FE	✓	✓		
Family FE			✓	✓
Observations	31851	31851	4078	4078
Families	26989	26989	2039	2039
R-squared	0.07	0.07	0.02	0.02
Panel 2: Aachen (unadjusted)				
Treated × Post1815	0.093***		0.345***	
	(0.008)		(0.042)	
Post1815	0.009**	0.009**	0.128***	0.128***
	(0.004)	(0.004)	(0.024)	(0.024)
Aachen × Post1815		0.115***		0.443***
		(0.009)		(0.046)
Muenster × Post1815		0.052***		0.160**
		(0.013)		(0.067)
City FE	✓	✓		
Family FE			✓	✓
Observations	46577	46577	4718	4718
Families	38741	38741	2359	2359
R-squared	0.04	0.04	0.06	0.06
Panel 3: Logit				
Treated × Post1815	1.196***		1.154	
	(0.073)		(0.104)	
Post1815	1.092***	1.092***	1.293***	1.293***
	(0.036)	(0.036)	(0.064)	(0.064)
Aachen × Post1815		1.091		1.056
		(0.078)		(0.109)
Muenster × Post1815		1.424***		1.400**
		(0.134)		(0.202)
City FE	✓	✓		
Family FE			✓	✓
Observations	46577	46577	4822	4822
Families	38741	38741	2411	2411

Notes: * p<0.1, ** p<0.05, *** p<0.01. We only include parents without national first given name. Clustered standard errors at the family level. Treated cities: Aachen and Muenster. Control city in Panel 1: Berlin. Control cities in Panel 2 and 3: Frankfurt (Main), Hanover, Heidelberg, Nuremberg, and Mannheim. Results based on equation 1.1. Data is collapsed in pre-and post period. In Panel 2, we do not adjust potentially "francicised" first names in Aachen.

Appendix B

'When autocrats fail: Bismarck's struggle against the socialists' (Chapter 2)

B.1 Tables and figures

TABLE B1
EFFECT OF ANTI-SOCIALIST LAWS

Dep. var.: Votes SAP	Baseline (1)	Controls (2)	Blue-Collar (3)
Forbidden Organization × 1871	-0.054**	-0.042**	-0.048**
	(0.020)	(0.019)	(0.022)
Forbidden Organization × 1874	0.007	0.012	0.011
	(0.011)	(0.011)	(0.013)
Forbidden Organization × 1877	0.022**	0.023**	0.014
	(0.010)	(0.010)	(0.010)
Forbidden Organization × 1881	-0.012	-0.015	-0.008
	(0.009)	(0.010)	(0.008)
Forbidden Organization × 1884	0.029**	0.022*	0.019
	(0.012)	(0.013)	(0.012)
Forbidden Organization × 1887	0.036***	0.023*	0.021
	(0.012)	(0.012)	(0.013)
Forbidden Organization × 1890	0.100***	0.081***	0.057**
	(0.017)	(0.017)	(0.021)
Further Controls		✓	✓
County FE	✓	✓	✓
Time FE	✓	✓	✓
Observations	1824	1824	1824
Counties	228	228	228
Elections	8	8	8
R-squared	0.35	0.39	0.42

Notes: The omitted election is 1877. The dependent variable measures the share of votes for the social democratic party. Treatment variables: share of newly insured population in 1884, interacted with time dummies. Controls: Population (log). Standard errors, clustered at the district level, in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
Sources: See Chapter 2.3.

TABLE B2
COMPARISON PRE-AND POSTPERIOD

Dep. var.: Votes SAP	Carrot (1)	Stick (2)	Interaction (3)
% Newly Insured × Post1883	1.275***		1.120***
	(0.227)		(0.208)
Post1883	-0.032***		-0.027***
	(0.007)		(0.007)
Forbidden Organization × Post1878		0.028**	
		(0.013)	
Post1878		-0.005	
		(0.008)	
% Newly Insured × Post1883			0.460**
× Forbidden Organization			(0.189)
Further Controls	✓	✓	✓
County FE	✓	✓	✓
Time FE	✓	✓	✓
Observations	456	456	456
Counties	228	228	228
Periods	2	2	2
R-squared	0.60	0.38	0.62

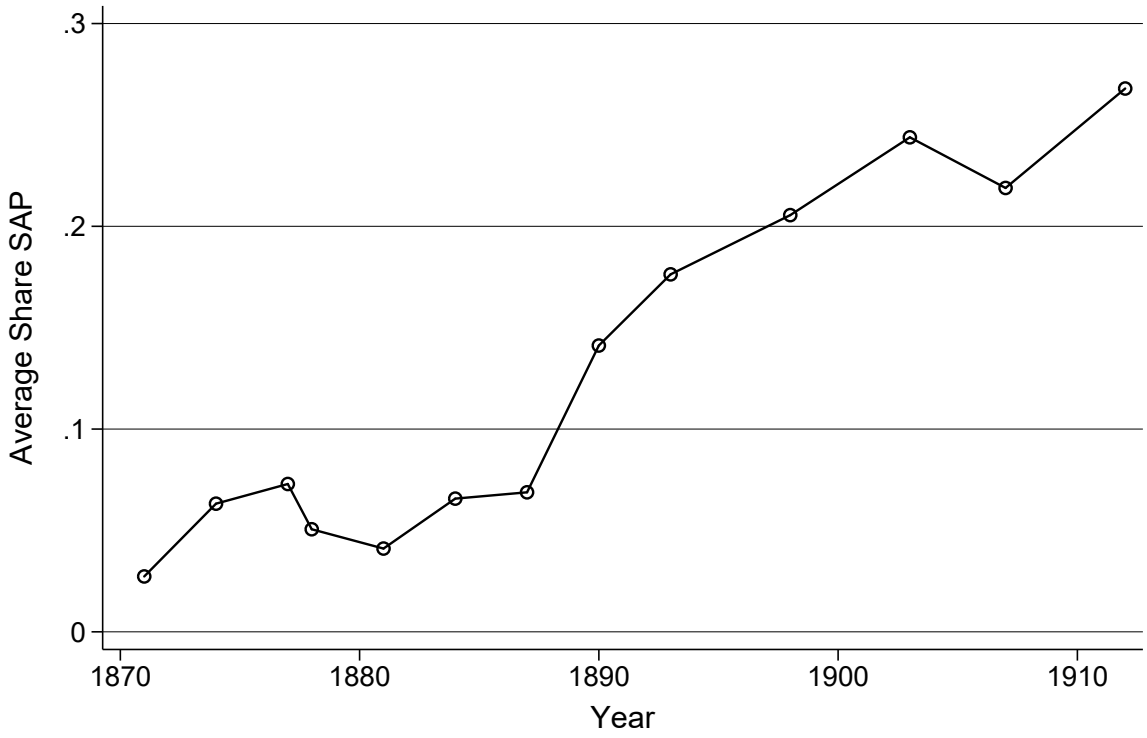
Notes: The dependent variable measures the share of votes for the social democratic party. Treatment variables: share of newly insured population in 1884; dummy variable for at least one forbidden organization in the wake of the anti-socialist law. Controls: population (log). Standard errors, clustered at the district level, in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
Sources: See Chapter 2.3.

TABLE B3
LONG-RUN EFFECTS OF THE SOCIAL INSURANCE

Dep. var.: Votes SAP	(1)
% Newly Insured × 1871	-0.568*
	(0.301)
% Newly Insured × 1874	0.027
	(0.311)
% Newly Insured × 1877	0.096
	(0.360)
% Newly Insured × 1878	-0.128
	(0.157)
% Newly Insured × 1884	0.806***
	(0.190)
% Newly Insured × 1887	0.875***
	(0.263)
% Newly Insured × 1890	1.628***
	(0.503)
% Newly Insured × 1893	1.657**
	(0.622)
% Newly Insured × 1898	1.562***
	(0.503)
% Newly Insured × 1903	1.214**
	(0.466)
% Newly Insured × 1907	1.258**
	(0.549)
% Newly Insured × 1912	1.593***
	(0.538)
Further Controls	✓
County FE	✓
Time FE	✓
Observations	2964
Counties	228
Elections	13
R-squared	0.66

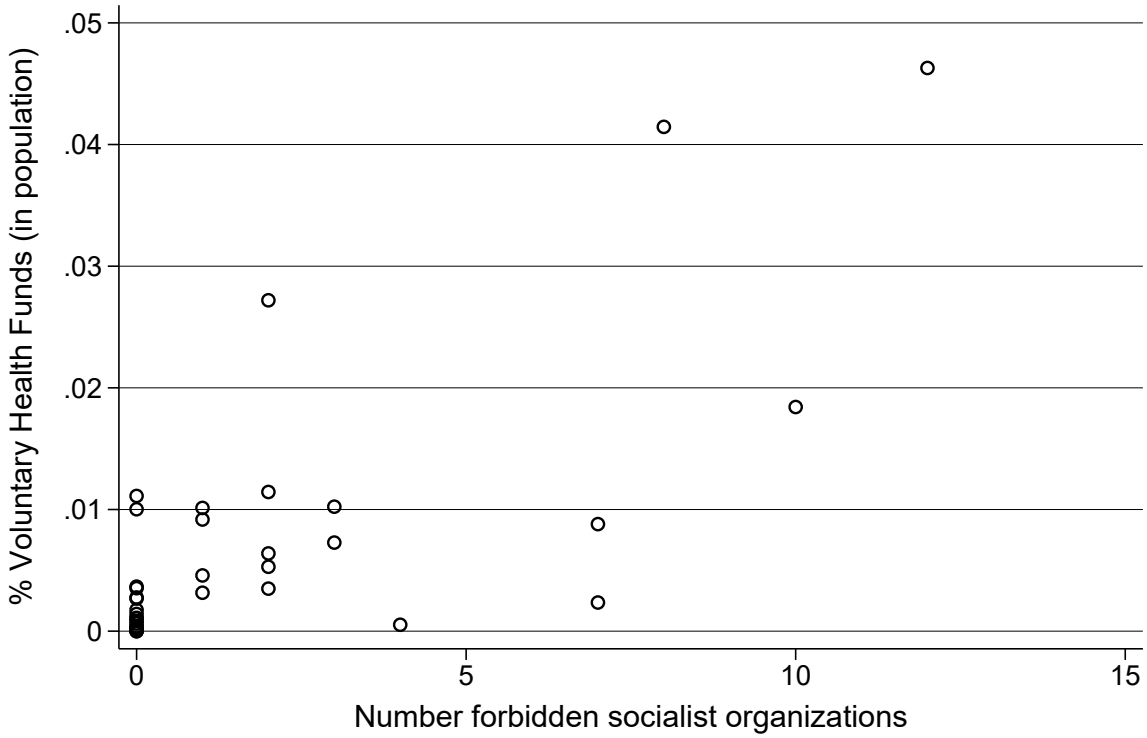
Notes: The omitted election is 1881. The dependent variable measures the share of votes for the social democratic party. Treatment variables: share of newly insured population in 1884, interacted with time dummies. Controls: Share of blue-collar workers interacted with time dummies, population (log). Standard errors, clustered at the district level, in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
Sources: See Chapter 2.3.

FIGURE B1
VOTE SHARES FOR THE SOCIALIST PARTY



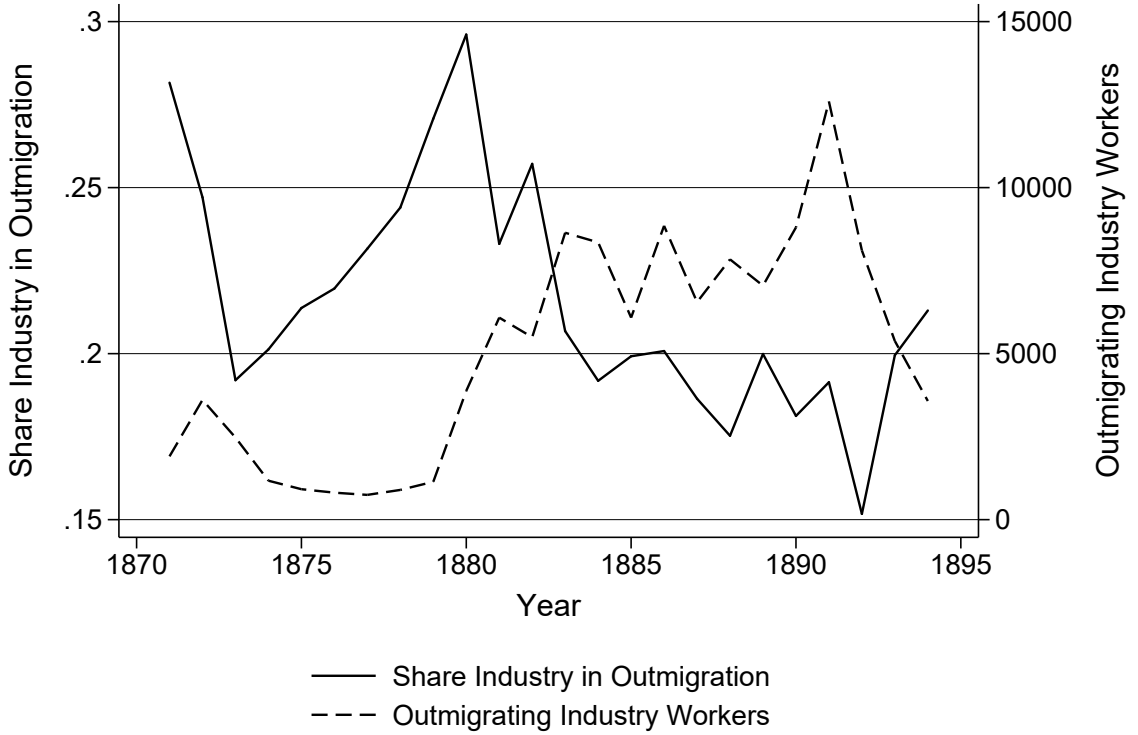
Notes: The graph shows the average vote share for the socialist party.
Sources: See Section 2.3.

FIGURE B2
VOLUNTARY HEALTH FUNDS AND FORBIDDEN SOCIALIST ORGANIZATIONS



Notes: The figure plots the number of forbidden socialist organizations (X-axis) and the share of people insured in voluntary health funds (Y-axis) for each district.
Sources: See Section 2.3.

FIGURE B3
INDUSTRY WORKERS AND MIGRATION



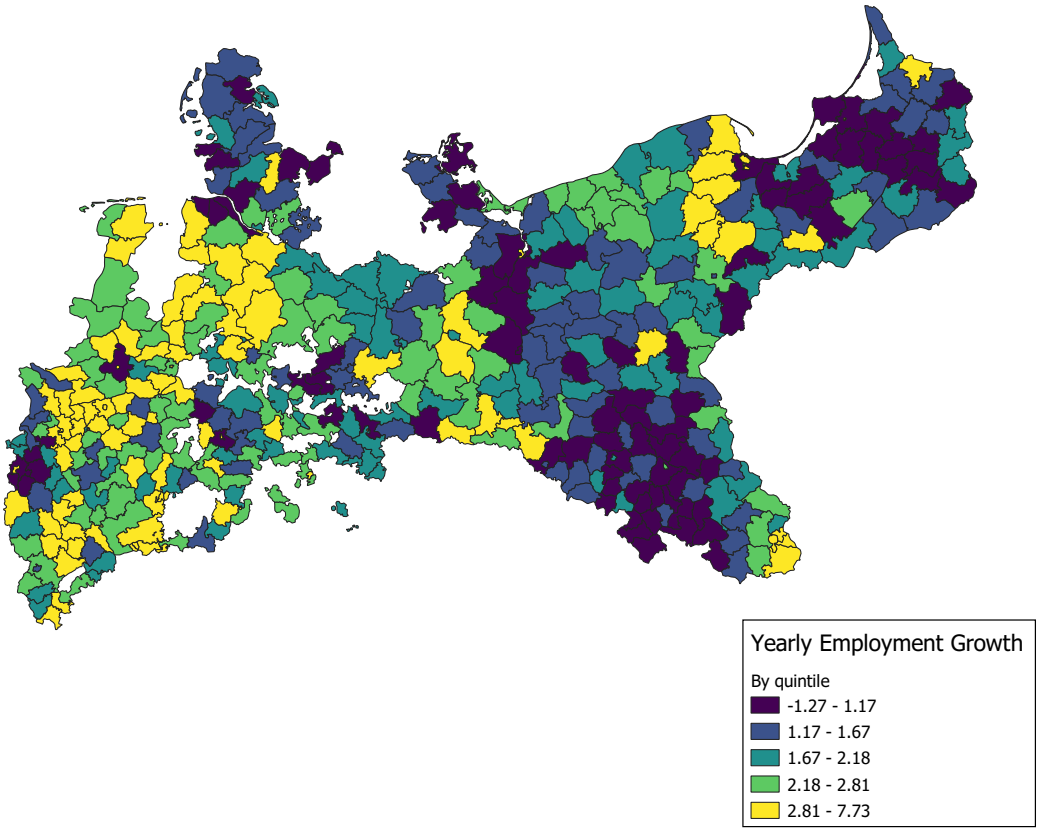
Notes: The figure shows the relative and absolute number of industry workers emigrating from the harbour of Hamburg.
Sources: See.

Appendix C

'Migrating from the grain invasion: Trade shocks, labor markets and political polarization in Imperial Germany, 1880-1913' (Chapter 3)

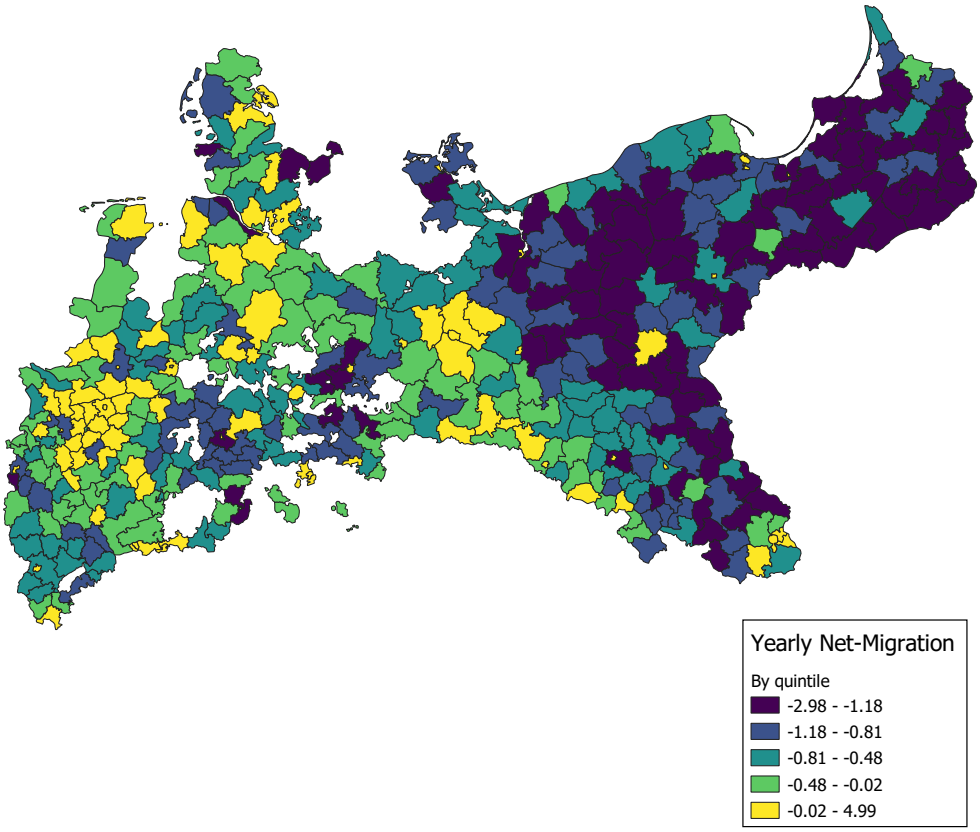
C.1 Tables and figures

FIGURE C1
GROWTH TOTAL EMPLOYMENT, 1895-1907



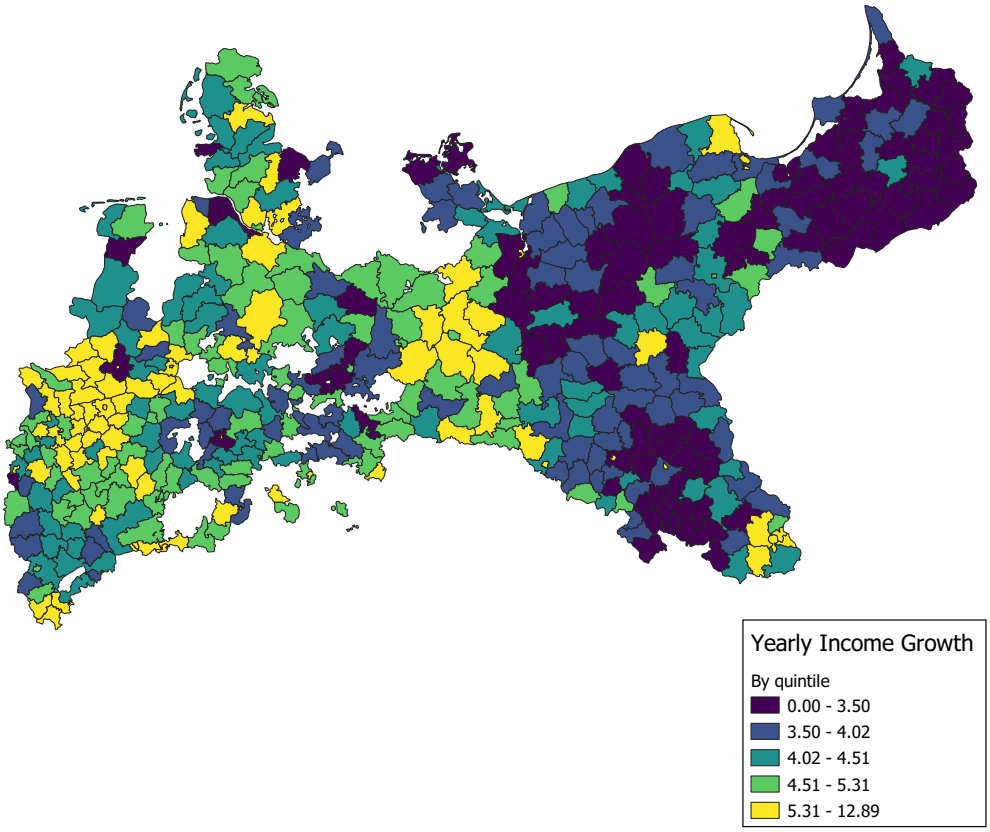
Notes: Yearly growth in total employment, 1895-1907.
Sources: See Section 3.2.

FIGURE C2
GROWTH NET-MIGRATION, 1895-1910



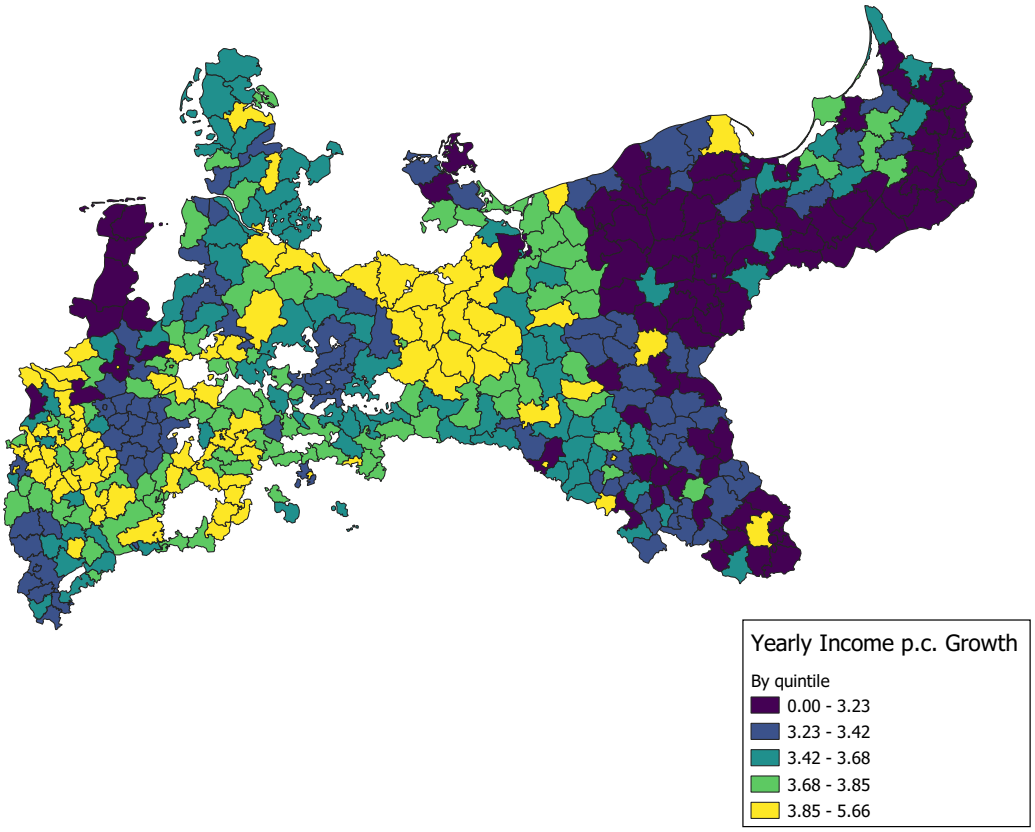
Notes: Yearly growth in net-migration, 1895-1910.
Sources: See Section 3.2.

FIGURE C3
GROWTH INCOME, 1895-1910



Notes: Yearly growth in income, 1895-1910.
Sources: See Section 3.2.

FIGURE C4
GROWTH INCOME PER CAPITA, 1895-1910

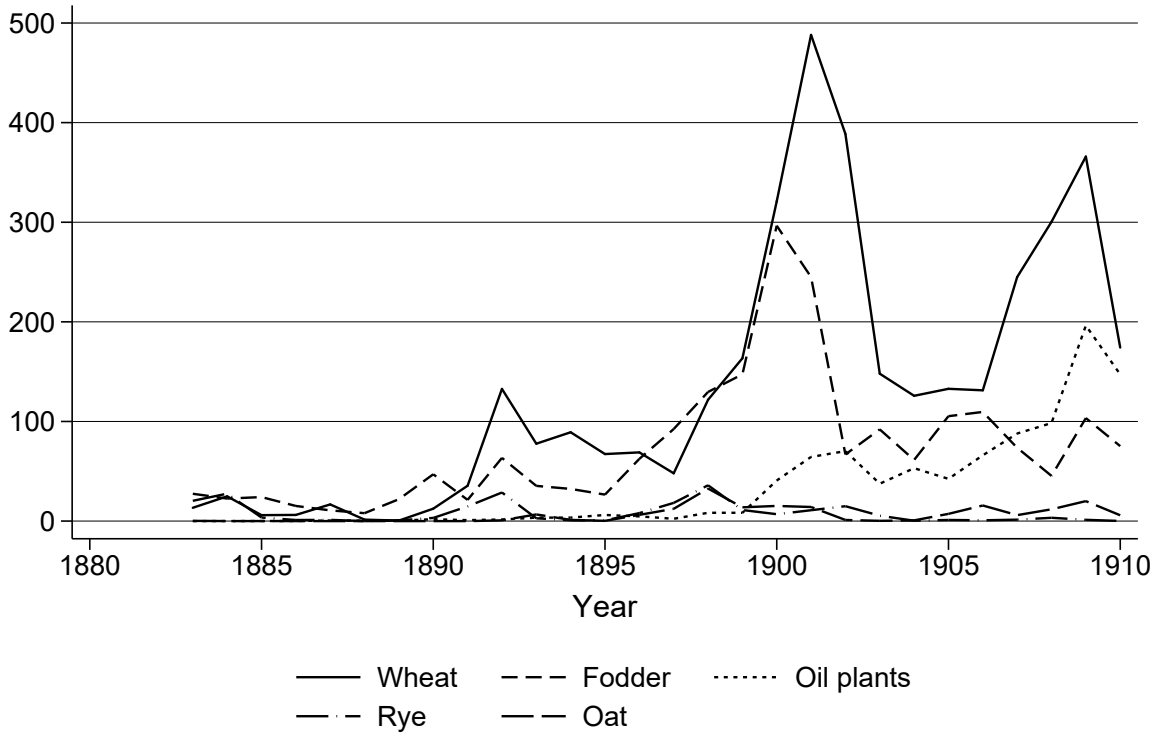


Notes: Yearly growth in income per capita, 1895-1910.
Sources: See Section 3.2.

FIGURE C5
NET-IMPORTS FROM US AND ARGENTINA, ITALY

Notes: The figure shows the net-imports from the US and Argentina to the US over time.
Sources: See Section 3.2.

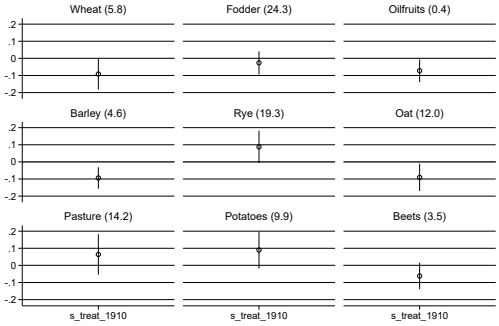
FIGURE C6
NET-IMPORTS FROM USA AND ARGENTINA BY CROPS, GERMANY



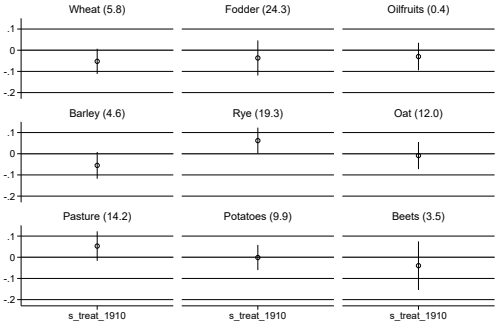
Notes: The figure shows the development of net-imports for different crops over time (in million Mark).
Sources: See Section 3.2.

FIGURE C7
CORRELATION CROPS AND OUTCOMES

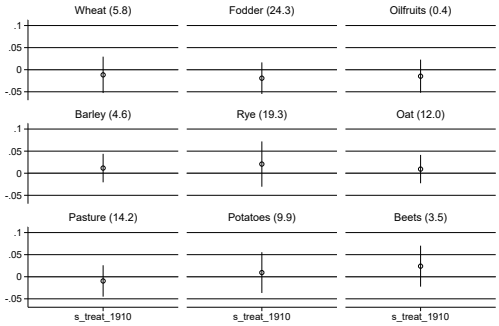
(A) EMPLOYMENT



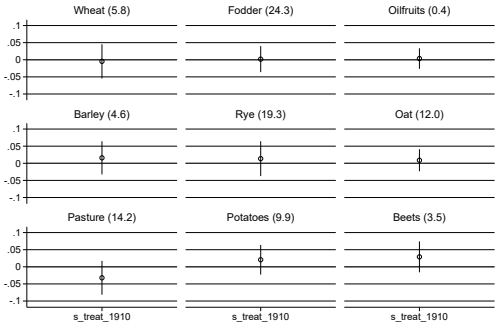
(B) MIGRATION



(C) INCOME



(D) INCOME P.C.



Notes: Standardized effect of share of different crops (as of 1883) on different dependent variable stated above the figure by period (first period as reference category). Average level of share of crops in parentheses. As controls we include share agricultural employment, horsepower per worker, distance to big city interacted with year dummies as well as county and time fixed effects.
Sources: See Section 3.2.

TABLE C1
DESCRIPTIVE STATISTICS

	(1) First Period 1880–1895	(2) Second Period 1895–1910
Shock agriculture	0.62	19.55
	(1.47)	(14.81)
% Emp agriculture	53.78	50.41
	(21.11)	(21.34)
Yearly employment growth	0.89	2.03
	(1.15)	(1.11)
Yearly migration growth	-0.64	-0.52
	(0.91)	(0.89)
Yearly income p.c. growth	-0.97	3.60
	(2.60)	(0.63)
Yearly income growth	-0.16	4.53
	(2.90)	(1.45)
% Land Big Farms	35.21	
	(23.65)	
Horsepower p.w.	0.08	
	(0.10)	
Distance to large city	87.60	
	(47.03)	
Observations	449	449

Notes: This table reports descriptive statistics by the two periods of our analysis. The standard deviation is provided in parenthesis.
Sources: See Section 3.2.

TABLE C2
EFFECT OF TRADE SHOCK ON EMPLOYMENT, MIGRATION, AND INCOME (OLS)

	(1)	(2)	(3)	(4)	(5)	(6)
	Panel 1					
	Employment Growth			Migration Growth		
Shock agriculture	-0.028***	-0.025***	-0.025***	-0.012***	-0.011***	-0.011***
	(0.005)	(0.004)	(0.004)	(0.002)	(0.002)	(0.002)
% Emp agriculture	-0.007*	-0.004	0.001	-0.024***	-0.023***	-0.023***
	(0.004)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)
% Land Big Farms			-0.001			-0.001
			(0.004)			(0.002)
Distance to large city			0.001			-0.001
			(0.002)			(0.001)
Horsepower p.w.			0.819**			-0.003
			(0.360)			(0.190)
R-squared	0.21	0.27	0.31	0.50	0.52	0.52
	Panel 2					
	Income Growth			Income p.c. Growth		
Shock agriculture	-0.015***	-0.012***	-0.016***	0.002	0.003	0.002
	(0.003)	(0.003)	(0.004)	(0.002)	(0.002)	(0.002)
% Emp agriculture	-0.037***	-0.033***	-0.027***	-0.009***	-0.006***	-0.008***
	(0.004)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)
% Land Big Farms			0.005			0.002
			(0.005)			(0.002)
Distance to large city			-0.001			-0.001
			(0.002)			(0.001)
Horsepower p.w.			0.746**			-0.336***
			(0.317)			(0.068)
R-squared	0.45	0.50	0.53	0.15	0.33	0.37
Region FE		✓	✓		✓	✓
Further Controls			✓			✓
Observations	449	449	449	449	449	449

Notes: Standard errors, clustered at the district level, in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Further controls include: distance to large city and share of land owned by large farm owners (more than 50 hectare).
Sources: See Section 3.2.

Table C3
First Stage Results

	Shock Agriculture		
	(1)	(2)	(3)
IV shock agriculture	6.407***	6.383***	6.277***
	(0.584)	(0.587)	(0.582)
% Emp agriculture	0.069**	0.056**	0.044
	(0.026)	(0.026)	(0.030)
% Land Big Farms			0.049***
			(0.018)
Distance to large city			0.008
			(0.009)
Horsepower p.w.			-1.319
			(0.857)
Region FE		✓	✓
Further Controls			✓
R-squared	0.85	0.86	0.86
Observations	449	449	449

Notes: Standard errors, clustered at the district level, in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Further controls include: distance to large city and share of land owned by large farm owners (more than 50 hectare).
Sources: See Section 3.2.

Table C4
Migration Response by Relative Trade Shock

Variable	All	Q1	Q2	Q3	Q4
Log Employment, 1895-1907	1.79	31.29	-1.65	-4.96	-17.90
Log Population, 1895-1910	13.35	28.98	12.49	7.00	4.81
Log "Natural" Population, 1895-1910	21.17	23.82	21.28	20.11	19.45
Log Migration, 1895-1910	-7.82	5.16	-8.79	-13.11	-14.65
Observations	449	113	112	112	112

Notes: This table reports unweighted averages of several variables by different subgroups. Column 1 includes all counties in the sample. Columns 2-5 group all counties into quartiles according to their relative exposure to agricultural trade shocks.
Sources: See Section 3.2.

Table C5
Migration by Skill

Province	(1) low skilled migration	(2) migrants among employed
Brandenburg (including Berlin)	76.05	31.55
Saxony	81.30	15.81
Westphalia	89.30	25.07
Rhineland	81.45	13.97
Schleswig-Holstein	81.31	20.21
Hanover	80.62	14.53
Hesse and Nassau	67.05	15.09
Pomerania	73.95	10.20
Poznan	64.30	8.02
Silesia	66.31	3.83
West Prussia	71.08	10.65
East Prussia	57.43	2.49

Notes: Column (1) indicates the share of low skilled migrants among all migrants in a province. Column (2) refers to the share of migrants among the workforce in a province. Migrant is defined here as worker not born in the same province as he/she was born.
Sources: See Section 3.2.

Table C6
Effect on Labor Income

	Labor Income Growth			Labor Income p.c. Growth		
	(1)	(2)	(3)	(4)	(5)	(6)
Shock agriculture	-0.016***	-0.014***	-0.020***	0.002	0.003	0.001
	(0.004)	(0.004)	(0.006)	(0.003)	(0.002)	(0.003)
% Emp agriculture	-0.040***	-0.034***	-0.026***	-0.012***	-0.008***	-0.008***
	(0.005)	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)
% Land Big Farms			0.006			0.001
			(0.005)			(0.003)
Distance to large city			-0.002			-0.002*
			(0.002)			(0.001)
Horsepower p.w.			0.841***			-0.246***
			(0.318)			(0.066)
F-Stat excl. inst.	120.30	118.25	116.49	120.30	118.25	116.49
Region FE		✓	✓		✓	✓
Observations	449	449	449	449	449	449

Notes: Standard errors, clustered at the district level, in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Further controls include: distance to large city, share of land owned by large farm owners (more than 50 hectare), horsepower per worker, share agricultural employment.
Sources: See Section 3.2.

TABLE C7
EFFECT OF TRADE SHOCK ON CHANGE IN DEATH RATE

	Change in Death Rate		
	(1)	(2)	(3)
Shock agriculture	0.011	0.011	-0.005
	(0.014)	(0.014)	(0.017)
F-Stat excl. inst.	120.30	118.25	116.49
Region FE		✓	✓
Further Controls			✓
Observations	449	449	449

Notes: Standard errors, clustered at the district level, in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Further controls include: distance to large city, share of land owned by large farm owners (more than 50 hectare), horsepower per worker, share agricultural employment.
Sources: See Section 3.2.

TABLE C8
MAIN RESULTS WITH INITIAL DISTRIBUTION OF CROPS

	Employment	Migration	Income	Income p.c.
	(1)	(2)	(3)	(4)
Shock agriculture	-0.031***	-0.010***	-0.017***	0.000
	(0.004)	(0.003)	(0.005)	(0.002)
F-Stat excl. inst.	297.21	297.21	297.21	297.21
Region FE	✓	✓	✓	✓
Further Controls	✓	✓	✓	✓
Observations	449	449	449	449

Notes: Standard errors, clustered at the district level, in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Further controls include: distance to large city, share of land owned by large farm owners (more than 50 hectare), horsepower per worker, share agricultural employment.
Sources: See Section 3.2.

TABLE C9
MAIN RESULTS WITH ELECTORAL CONSTITUENCIES

	Employment	Migration	Income	Income p.c.
	(1)	(2)	(3)	(4)
Shock agriculture	-0.047***	-0.025***	-0.043***	-0.009
	(0.008)	(0.005)	(0.012)	(0.007)
F-Stat excl. inst.	93.73	93.73	93.73	93.73
Region FE	✓	✓	✓	✓
Further Controls	✓	✓	✓	✓
Observations	225	225	225	225

Notes: Standard errors, clustered at the district level, in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Further controls include: distance to large city, share of land owned by large farm owners (more than 50 hectare), horsepower per worker, share agricultural employment.
Sources: See Section 3.2.

Appendix D

'Weber revisited: The Protestant Ethic and the Spirit of Nationalism' (Chapter 4)

D.1 Sources

Table D1
Data Sources

Variable	Description	Years	Source
Religion			
Protestants	Share Protestants	1875, 1880, 1890, 1900, 1905	Galloway (2007)
Other religions	Share other religions, i.e. non-Catholic and non-Protestant	1875, 1880, 1890, 1900, 1905	Galloway (2007)
Reformed Protestants	Share reformed Protestants	1871	Königlich Statistisches Bureau (1875)
Literacy by Religion		1871	Galloway (2007)
Savings			
Number of savings-banks	Number of savings-banks	1875, 1882, 1888, 1898, 1904	Lehmann-Hasemeyer and Wahl (2017)
Deposits in savings-banks	Ammount of deposits at savings-banks	1875, 1882, 1888, 1898, 1904	Lehmann-Hasemeyer and Wahl (2017)
Deposits in Polish credit co-operatives	Amount of deposits (equity and borrowed capital) at Polish credit cooperatives	1907	Politische Abteilung des königlichen Polizeipräsidiums in Posen (1909)
Instrumental Variable			
Ecclesiastical status	1 if data of Cantoni (2012) indicate that a prince-bishop or another clergyman ruled over the area		Spenkuch (2017) and Spenkuch and Tillmann (2018)
Reichsmatrikel	Contribution to the Imperial War Tax	1521	Zeumer (1913)

See next page

Variable	Description	Years	Source
Protestant in 1624	1 if the prince who reigned over the corresponding area in 1624 decided to choose Protestantism over Catholicism	1624	Spenkuch (2017) and Schindling and Ziegler (1989, 1992, 1993b,a, 1995, 1996)
Printing Press	1 if data of Rubin (2014) indicate that at least one of the cities in a given county had a printing press at the beginning of the sixteenth century		
Latitude	Based on the coordinates of a counties' centroid		Own calculations using QGIS
Distance to Wittenberg	Distance to the city of Wittenberg		Own calculations using QGIS
Ethnic Nationality			
Mother tongue German	Share mother tongue German	1867 (partially), 1890	Galloway (2007), Belzyt (1998)
Further Controls			
Labor imcome per capita	Estimated by multiplying sector employment and wage data	1882, 1895, 1907	Kaiserliches Statistisches Amt (1884, 1897, 1910); Hoffmann (1965); Becker *et al.* (2014)
Average household size	Households divided by population	1875, 1880, 1890, 1900, 1905	Galloway (2007)
Urban population	Share urban population	1875, 1880, 1890, 1900, 1905	Galloway (2007)
Potential male working population	Share men above 15	1875, 1880, 1890, 1900, 1905	Galloway (2007)

See next page

Variable	Description	Years	Source
Polish doctors	List of all doctors in the province Poznan	1905	Verband der Ärzte Deutschlands (1908)
% age below 10, % Jews, % females, % born in municipality, % of Prussian origin, average household size, population size (log), population growth 1867-1871 (in %), % missing education info, % blind, % deaf-mute, % insane		1871	Becker and Woessmann (2009)

D.2 Tables and figures

TABLE D2
LUTHERANS AND REFORMED PROTESTANTS

	(1)	(2)
Panel 1: Dep. Var. Savings per Capita, 1875-1905		
Share Protestants	0.071	
	(0.081)	
Share Reformist		-0.029***
		(0.009)
Share Lutherans		-0.074*
		(0.041)
Further Controls	Y	Y
N	2170	2170
R^2	0.359	0.356
Panel 2: Dep. Var. Literacy, 1871		
Share Protestants	0.099***	
	(0.010)	
Share Reformist		0.173**
		(0.082)
Share Lutherans		0.129***
		(0.014)
Further Controls	Y	Y
N	452	452
R^2	0.737	0.735

Notes: In Panel 1 Standardized beta coefficients. Standard errors in parentheses. Robust standard errors clustered at the province level. Further controls include in Panel 1: number of savings banks, share working men above 14, share urban population, average household size, share other religions, dummy for counties w/o savings bank. Further controls include in Panel 2: % age below 10, % Jews, % females, % born in municipality, % of Prussian origin, average household size, population size (log), population growth 1867-1871 (in %), % missing education info, % blind, % deaf-mute, % insane. * p<0.1, ** p<0.05, *** p<0.01.
Sources: See Table D1.

TABLE D3
POLISH SAVINGS, ROBUSTNESS

	(1)	(2)	(3)	(4)
	Savings p.c.		+ Pol. credit coop.	
Share Protestants	0.149	0.141	0.149	0.140
	(0.117)	(0.118)	(0.118)	(0.119)
Share German Speaking	0.167***	0.177**	0.152**	0.163**
	(0.049)	(0.057)	(0.051)	(0.058)
Income per Capita		-0.047		-0.048
		(0.075)		(0.075)
Further Controls	Y	Y	Y	Y
N	338	338	338	338
R^2	0.290	0.291	0.282	0.284

Notes: Standardized beta coefficients. Standard errors in parentheses. Robust standard errors clustered at the province level. Further controls: number of savings banks, share working men above 14, share urban population, average household size, share other religions, dummy for counties w/o savings bank. We exclude the provinces of Silesia and East Prussia. * p<0.1, ** p<0.05, *** p<0.01.
Sources: See Table D1.

TABLE D4
POLISH DOCTORS IN THE PROVINCE POZNAN, 1907

County	% Polish Doctors	% Mothertongue Polish
District Poznan		
Wreschen	0.44	0.78
Pleschen, Jarotschin	0.29	0.79
Schroda	0.13	0.84
Schrimm	0.33	0.78
Kosten, Schmiegel	0.50	0.83
Graetz	0.29	0.65
Neutomischel	0.25	0.65
Posen (Stadt)	0.38	0.47
Posen (Land)	0.28	0.70
Obornik	0.40	0.58
Samter	0.30	0.70
Birnbaum, Schwerin	0.27	0.31
Meseritz	0.20	0.20
Bomst	0.30	0.47
Fraustadt, Lissa	0.23	0.31
Gostyn, Rawitsch	0.36	0.68
Koschmin, Krotoschin	0.43	0.70
Adelnau, Ostrowo	0.44	0.81
Kempen, Schildberg	0.36	0.85
District Bromberg		
Czarnikau, Filehne	0.06	0.27
Kolmar	0.30	0.17
Wirsitz	0.25	0.46
Bromberg	0.10	0.28
Schubin, Znin	0.36	0.63
Hohensalza, Strelno	0.35	0.68
Mogilno	0.30	0.68
Gnesen, Witkowo	0.39	0.68
Wongrowitz	0.38	0.71

Notes: We identify Polish doctors via their last name. Every person with "ski" or "cz" or "zki" or "sz" or "tz" or "yz" or "zc" or "ow" or "wy" or "zy" in his lastname is classified as Polish doctor.
Sources: See Table D1.

TABLE D5
EFFECT OF PROTESTANTISM ON LITERACY, 1871 (USING DISTANCE TO WITTENBERG AS IV)

	(1)	(2)	(3)	(4)	(5)
Panel 1: OLS	Dep. Var. Literacy				
Share Protestants	0.099***	0.062***	0.057***	0.033***	0.040***
	(0.010)	(0.008)	(0.009)	(0.007)	(0.007)
Share German Speaking		0.221***			0.213***
		(0.014)			(0.016)
R^2	0.737	0.831	0.610	0.431	0.735
Panel 2: Second Stage	Dep. Var. Literacy				
Share Protestants	0.189***	0.149***	0.182***	0.077***	0.145***
	(0.028)	(0.026)	(0.024)	(0.014)	(0.021)
Share German Speaking		0.178***			0.173***
		(0.020)			(0.022)
Panel 3: First Stage	Dep. Var. Protestantism				
Distance to Wittenberg in km	-0.095***	-0.086***	-0.114***	-0.140***	-0.109***
	(0.011)	(0.011)	(0.012)	(0.015)	(0.012)
Share German Speaking		0.404***			0.170
		(0.073)			(0.106)
R^2	0.419	0.457	0.448	0.620	0.452
Including Eastern Provinces	Y	Y	Y	N	Y
Only Holy Roman Empire	N	N	Y	Y	Y
Further Controls	Y	Y	Y	Y	Y
F-Stat excluded instrument	74.19	64.21	96.12	89.27	85.32
N	452	452	378	280	378

Notes: Standard errors in parentheses. IV: Distance to Wittenberg. We show in Section 4.4 that distance to Wittenberg is not suitable as an instrumental variable, because it violates the exclusion restriction. Eastern provinces include East and West Prussia, Poznan, and Silesia. Further controls include: % age below 10, % Jews, % females, % born in municipality, % of Prussian origin, average household size, population size (log), population growth 1867-1871 (in %), % missing education info, % blind, % deaf-mute, % insane.. * p<0.1, ** p<0.05, *** p<0.01. Note that the different standard errors between panel 1 column 5 in this table and Table 4.5 panel 1 column 3 are due to the use of clustered standard errors in Table 4.5. In this table, we rely on the exact specification by Becker and Woessmann (2009).
Sources: See Table D1.

TABLE D6
LITERACY AS MEDIATOR, 1871

	(1)	(2)	(3)	(4)	(5)
	Dep. Var. Income Tax per Capita				
Total effect	-0.199	-0.315	0.835***	-0.246	0.842***
	(0.201)	(0.330)	(0.272)	(0.189)	(0.278)
Direct effect	0.187*	0.184	0.083	0.272*	0.144
	(0.103)	(0.143)	(0.083)	(0.152)	(0.088)
Indirect effect (literacy)	-0.039	-0.500	0.752***	-0.517*	0.698***
	(0.104)	(0.434)	(0.291)	(0.303)	(0.270)
Mediator explains	19.87%	158.42%	90.09%	210.57%	82.93%
Further controls	Y	Y	Y	Y	Y
Only HRE	Y	Y	N	N	Y
Including eastern provinces	Y	N	Y	N	Y
IV	Residual Decision		Distance to Wittenberg		
N	356	261	426	284	356

Notes: Standard errors in parentheses. Eastern provinces include East and West Prussia, Poznan, and Silesia. * p<0.1, ** p<0.05, *** p<0.01. Further controls: % Jews, % females, % age below 10, % of Prussian origin, average household size, population size (log), population growth 1867-1871 (in %), % missing education info. Method: mediation analysis with IV (Dippel *et al.*, 2018). For the computation we rely on the Stata package *ivmediate* provided by Dippel *et al.* (2020a).
Sources: See Table D1.

TABLE D7
SPILL-OVER EFFECT OF PROTESTANTISM ON LITERACY AMONG CATHOLICS, 1871

	(1)	(2)	(3)
Panel 1: OLS	Dep. Var. Literacy among Catholics		
Share Protestants	0.020	-0.000	-0.000
	(0.030)	(0.019)	(0.019)
Share German Speaking	0.269**	0.324**	0.324**
	(0.093)	(0.121)	(0.121)
R^2	0.667	0.525	0.525
Panel 2: Second Stage	Dep. Var. Literacy among Catholics		
Share Protestants	0.273	0.199*	-0.057
	(0.179)	(0.096)	(0.043)
Share German Speaking	0.145*	0.248*	0.346**
	(0.077)	(0.123)	(0.122)
Panel 3: First Stage	Dep. Var. Protestantism		
Distance to Wittenberg in km	-0.086**	-0.109***	
	(0.038)	(0.032)	
Residual Decision 1624			45.369***
			(2.866)
Share German Speaking	0.404**	0.170	0.327**
	(0.160)	(0.160)	(0.144)
R^2	0.457	0.452	0.489
Further Controls	Y	Y	Y
Only Holy Roman Empire	N	Y	Y
N	452	378	378

Notes: Standard errors in parentheses. Further controls include: % age below 10, % Jews, % females, % born in municipality, % of Prussian origin, average household size, population size (log), population growth 1867-1871 (in %), % missing education info, % blind, % deaf-mute, % insane. * p<0.1, ** p<0.05, *** p<0.01.
Sources: See Table D1.

D.3 Protestantism and literacy rates: Direct comparison with Becker and Woessmann (2009)

The well-known paper by Becker and Woessmann (2009) – further BW – serves as an important reference point for our work. Therefore, we compare our findings with their main results along several dimensions.

First, we compare our main results from the cross-table and the 2SLS regression with the main findings from the IV-regression by BW. Using the cross-table, we find that Protestants are on average only slightly over-proportionally literate (one percentage point for the complete sample and 0.15 percentage points once we exclude the eastern provinces).[54] In our IV specification (Table 4.5), we find no significant effect of literacy. Moreover, our results from the cross-table show that the difference in literacy by religion is much bigger for regions in the East where the Polish minority lived (Figure 4.5). These findings stand in contrast to the results from Becker and Woessmann (2009, p.558): "In fact, the point estimate is significantly higher, with a difference in literacy of 18.9 percentage points between an all-Protestant and an all-Catholic county." We exactly replicate their finding (column 2 in Table III in Becker and Woessmann (2009, p.559)), using distance to Wittenberg as an instrument, in column 1 in Table D5. Thus, there seems to be a striking difference between, on the one hand, the relationship between Protestantism and literacy what BW measure, and, on the other hand, comparing literacy by religion based on the cross-table and our 2SLS results. Comparing Table D5 column 5 with Table 4.5 column 3, we see that the discrepancy in the second stage results stem from the use of different IVs. These different results need an explanation. One possibility is that the instrumental variable used by BW may violate the exclusion restriction because the IV is highly correlated with German share as discussed in section 4.4 in the text. Hence, BW probably overestimate the effect of Protestantism on literacy due to their choice of instrument. Distance to Wittenberg does not isolate the effect of Protestantism because it captures both, the effect of Protestantism and that of ethnicity. Instead, the IV based on the idiosyncratic part of a rulers' choice to become Protestant in the 17th century is less likely to suffer from such a violation of the exclusion restriction. The main reason is that this instrument controls for distance to Wittenberg and latitude, which should capture among other things also variation in ethnic heterogeneity. We have also tested for this formally following Conley *et al.* (2012) and Karadja and Prawitz (2019), see section 4.4. In the context of abating religious tensions and growing ethnic conflict at the end of the nineteenth-century, this is crucial.

Second, we provide a mediation analysis on literacy rates, income and Protestantism. We rely on a new approach for a causal mediation analysis put forward by Dippel *et al.* (2018) and use STATA code by Dippel *et al.* (2020a). This methodology allows us to calculate the share of the total effect of Protestantism on economic outcomes that can be attributed to higher literacy

[54] Note that Becker and Woessmann (2009, p.549) mention the cross-table but do not provide an analysis based on this cross-table.

among Protestants. BW show that higher income tax per capita for Protestant regions is due to higher literacy as crucial mediating factor. We replicate this finding by relying on the IV mediation analysis put forward by Dippel *et al.* (2018). If we use the IV from BW in column 3 in Table D6, we find that indeed about 90% of the overall effect of Protestantism on income tax per capita is due to literacy. If instead we use our preferred IV as described above the results change: income tax per capita is no longer significantly related to Protestantism and literacy is no longer a crucial mediating factor (columns 1 and 2 in Table D6). This is an example that the context can be essential for econometrics: because the geography of ethnic differences overlaps with that of religious differences, a simple distance-based instrument is likely to violate the exclusion restriction.

Selbstständigkeitserklärung

Ich versichere, die von mir vorgelegte Dissertation selbstständig und ohne unerlaubte Hilfe und Hilfsmittel angefertigt, sowie die benutzten Quellen und Daten anderen Ursprungs als solche kenntlich gemacht zu haben.

Ich bezeuge durch meine Unterschrift, dass meine Angaben über die bei der Abfassung meiner Dissertation benutzten Hilfsmittel, über die mir zuteil gewordene Hilfe sowie über frühere Begutachtungen meiner Dissertation in jeder Hinsicht der Wahrheit entsprechen.

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Berlin, 12. August 2020